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THE PSYCHOLOGICAL REVIEW

THE QUESTION OF ASSOCIATION TYPES[1]

BY FREDERIC LYMAN WELLS

In dealing with free association material it is often necessary to speak of the associations of different individuals as running along certin specified lines, or of showing certain types (*Sachlicher Typus*, *Konstellationstypus*, etc.). However, there exists no very definite knowledge of how consistently such reactional tendencies are preserved, and of how far any given series of observations with a single individual may be indicative of that individual's type of response to the association test.[2] With a view to contributing to a better understanding of these matters there were undertaken the calculations that form the basis of the present report.

The experimental material is largely that discussed from other standpoints elsewhere; it includes a total of 10,900 observations, with 28 different subjects, 11 men and 17 women, hospital nurses with the exception of two physicians. For further account of the experimental conditions the reader may be referred to the previous discussions.[3]

The working concept of individual differences, for the association test and for measurements in general, is broadly based upon the postulate that any given individual preserves, within the function measured, a range of variability that is less than the variability of normal individuals about the

[1] From the McLean Hospital, Waverley, Mass.

[2] The study most nearly related to the present is that of Fürst, *Journal für Psychol. u. Neurol.*, IX., 1907, 243–278.

[3] 'Practise Effects in Free Association.' *American Journal of Psychology*, Vol. XXII., pp. 1–13. 'Some Properties of the Free Association Time.' PSYCHOLOGICAL REVIEW, Vol. XXIII., pp. 1–23.

normal average. In the absence of external factors such as practise, individuals must vary from themselves at different times less than they do from each other; and even in the equal presence of such factors, the individual relationships must still be preserved. To the extent to which these conditions are satisfied, we may speak of clear-cut individual differences in our various functions.

The variation of the same individual from time to time may be termed the *personal variation*, as distinguished from his variation from similar measurements of other individuals. The comparison of this *personal variation* with the mean variation of a group affords an everywhere interesting psychological datum. In some previous experiments, it thus appeared that individuals varied relatively much less among themselves and more among each other, in the preference for certain sorts of pictures than in the judgment of color-differences or of weights.[1] Indeed, if the result quoted for the weights were generally valid, the accuracy of weight perception would be of almost negligible value as a factor of individual difference. It is certainly desirable that we should have clear knowledge of the relationship of the personal variation to the group variation in all important psychological measures.

If no constant deviations, such as practise effects, are indicated, it is probably sufficient to know the comparative magnitudes of the mean variations of the function taken personally and for the groups: but where such factors are obviously present, the demands of the situation are much better met by the correlation methods. The three functions of the association experiment that appear for consideration in these respects are (1) the reaction time, (2) the tendency to give common or specialized responses (Kent and Rosanoff), (3) the relational character of the responses with reference to the stimulus word, as given in the quasi-logical methods of classifying the responses (Aschaffenburg, Münsterberg, Wreschner, Jung, et al.).

During the latter part of October, 1909, twenty-five

[1] 'On the Variability of Individual Judgment.' Essays in Honor of William James, 511–549.

subjects, ten men and fifteen women, underwent the association experiment of one hundred stimulus words prepared and standardized by Kent and Rosanoff. Fourteen months later, in December, 1910, such of these subjects as were still available, fourteen in number, underwent the experiment a second time. We are concerned here with the comparison of the records made by the different subjects with those made by the same subject on the different occasions.

The median association times for the one hundred associations of each individual in each subject compare as follows in the two experiments. The time unit, here as elsewhere, is one-fifth of a second.

Subj.	Oct., 1909	Dec., 1910
I.	22.0	7.6
II*	14.8	8.0
III.	8.3	8.2
IV.	8.8	8.5
V*	19.0	15.6
VI.	8.2	6.1
VII*	13.4	7.8
VIII.	12.8	11.0
IX.	8.7	7.2
X.	8.0	6.3
XI.	7.7	6.6
XII.	7.8	6.9
XIII.	10.2	8.3
XIV.	8.6	7.3

The subjects marked with an asterisk are not included in these immediate calculations because of extensive intervening practise, though it is not absolutely certain that V. should have been excluded on this ground. The record is in other ways an exceptional one.

The enormous difference in the performances of subject I. is probably less important than it looks. Every other experience with the test indicates it to be one of those unfortunate experimental accidents that result from some factor, uncontrolled, because insufficiently appreciated; the second experiment is probably the representative one. It may be remarked in passing that the type of association is little different in the two tests with this subject; indeed, the actual response words were in 47 per cent. of the cases the same as before.

The generally shorter times of the second experiment may and probably should be taken as the product of better adaptation to the test. It is true that there is considerable intervening opportunity for practise in manipulating the stopwatch, but the changes are not such as would be induced in this way. It is not likely that the different médians would be thus affected in so different degrees. Very long times are also much less frequent in the second experiment than in the first. Of those reaction times of 50 (10 seconds) and over in length, the unpractised subjects had 37 in the first experiment, 9 in the second. In the matter of a better experimental adaptation, this fact means considerably more than the slight decrease in the medians.

The uniform shortening of the time limits greatly the significance of the comparative mean variations. For the unpractised subjects, the average time is 10.1 in the first experiment, and 7.6 in the second; while the mean variations of these two averages are 2.8 and 1.0 respectively. The average personal variation between the two experiments is 1.2, if the anomalous first case be included, .64 without it.[1] If the former be compared with the group mean variation for the first experiment, and the latter with the second experiment, it is seen that, in spite of the uniform decrease in time, the personal variations are smaller by about one third.

For the reasons indicated, however, the correlation methods furnish the more logical evidence. It is altogether distorted here by the practised subjects, and especially the anomalous case. If the practised subjects alone be excluded, there is still no correlation worth mentioning, the Pearson approximating .28; but if the anomalous case be also ruled out, the Pearson of the remaining ten figures out at .86. This is surely an extreme instance of distortion by a single anomalous observation, which it is presumably more accurate to exclude. In the remaining cases, the tendency to preserve the type in reaction time is fairly definite.

In calculating the reaction times of the second series of experiments, it was thought worth while to observe how much

[1] With correction for constant error, this quantity becomes .27.

the two halves of the experiment might differ in this respect. Fifty observations should here give a fairly reliable median, and we should be assured of a greater constancy of experimental attitude than in experiments separated by a wide interval. The times have a fairly uniform tendency to run longer in the second half of the experiment; it would be interesting to determine if there were a fatigue phenomenon here, as might perhaps be done with larger series of stimulus words; in this instance it is quite possible that the second fifty of the words chance to be more difficult.

Before leaving this phase of the matter, attention may be called once more to the character of the practise curves reproduced in a previous contribution.[1] In the median of fifty reaction times, the differences between four of the subjects are seen to be entirely definite; the practise curves of RED, BROWN, BLACK and BLUE, never touching except for one intersection of the last two. This evidence supports that of the figures just given, that clear-cut individual differences are to be found in the reaction time of the association experiment. It is less certain that this reaction time is determined in such a way as to make a particularly significant psychological measure. Like a high cephalic index, a long reaction time for an individual may be quite definitely made out, but both may mean absolutely different things according to the setting in which they occur.

It is but natural that in the second test a stimulus word frequently elicits the same response as in the first test. The number of responses thus repeated to the same stimulus word has found some application in pathological cases, the practise being to rate the subject higher, the more responses were changed. This interpretation is scarcely indicated for it here. The figures are as follows:

(Number of responses identical in each experiment)

Subject	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	XIII	XIV
First half	24	7	12	21	8	19	0	8	24	22	21	23	22	26
Second half	23	18	21	21	10	30	5	14	14	31	25	21	29	28
Total	47	25	33	42	18	49	5	22	38	53	46	44	51	54

[1] *American Journal of Psychology*, XXII., plate facing page 2.

The range is quite extensive and a form of distribution is distinct. Excluding the practised subjects, the personal variation in this function compares directly with the group variation in the ratio of about 5½ to 7; that is, individual differences among these subjects are not very well defined here. There appears, however, some correspondence of the tendency to differentiate the responses *in toto* with a large number of predicate associations, as well as with a tendency to individualized responses.

The simplest criterion of the egocentricity of a Kent-Rosanoff record is the number of 'individual' reactions, that is, as Kent and Rosanoff use the term, the number of given responses that do not occur in their frequency tables (or for a subject that is concerned in these tables, occur only the one time that that subject gave them). Through analyzing large masses of material in this way, it is doubtless possible to reach a number of interesting and useful conclusions. The number of these 'individual' reactions occurring in the first and second experiments is as follows in the two tests:

Subject	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	XIII	XIV
No. ind. react. 1st. exp..	0	9	2	3	4	2	17	1	4	2	2	2	8	0
No. ind. react. 2d exp...	0	5	10	1	15	5	31	9	4	2	1	0	3	3

For all subjects, the averages are 4.0 individual reactions in the first experiment, and 6.3 in the second experiment. The group variation is 3.2 in the first, and 5.5 in the second. The average personal variation between the two experiments is 2.1. The correlation is low, being but 65 per cent. of plus signs from the median. Excluding the practised subjects, the individual differences become even less distinct, the personal variation being as great as the group variation of the first experiment.

The failure of this measure for the present calculations is due entirely to its not being sufficiently fine-drawn. It has the advantage of being rapidly determined, and perhaps suffices for comparisons between considerable groups of subjects; but in comparisons between different individuals, and

especially between different records of the same individual, it is not only much too coarse, but may be actually misleading.

For such purposes, the detailed analysis of the data of the frequency tables amply repays the time that it requires. Each association reaction has a certain value from 0 up to a possible 1,000 and an actual 650, according to its place in the frequency tables.[1] For each record may be calculated a *coefficient of community*, this being the median value of all the associations in that record. This figure varies within wide limits for different individuals. If it is high, it indicates a tendency to respond with common, ordinary reactions; if low, it indicates a tendency towards specialized and individualized responses. This coefficient of community, which may be abbreviated as *c*, is as follows for the different subjects:

Subject	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	XIII	XIV
c, 1st exp. . .	16.5	2.2	9.0	11.5	15.1	8.5	2.2	6.3	6.6	17.4	9.5	15.2	9.3	11.0
c, 2d exp. . . .	15.1	3.0	6.0	10.0	2.0	7.6	.7	3.3	9.0	18.0	12.0	13.5	7.2	9.3

The most cursory glance at these figures shows that there is in reality a high degree of inherency in the tendency to give common or specialized responses, which the mere number of individual reactions altogether fails to indicate. The mean group variations of the first and second experiments are 3.8 and 4.1 respectively. The personal variation between the experiments is 1.3 including the anomalous Case V. There has here been a far-reaching alteration in the form as well as in the individuality of the associations. Without this case the group variations would approximate 3.7 and 3.9, the personal variation being .9. In the correlation methods, the only negative case from the median is that of V.; elsewhere the results with and without it are as follows:

	Incl. V.	Excl. V.
Order displacement.....	18 per cent.	9 per cent.
Pearson.....	.73	.83

In this function therefore, we appear to possess a measure of individual differences that will bear comparison with any

[1] Kent and Rosanoff, 'A Study of Association in Insanity,' *American Journal of Insanity*, Vol. XXVII., pp. 40, 48–96.

other psychological measure of equal complexity and significance.

The median of community has been calculated separately for the responses which are the same in the two experiments and for those which are different. The figures show some points worth bringing out and are as follows:

Subject	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	XIII	XIV
c repeated responses.....	25.7	6.2	14.7	30.6	14.1	18.4	12.5	16.5	16.6	24.6	21.3	21.7	17.2	15.6
c changed responses (1) ..	10.4	1.4	4.6	5.2	15.2	4.6	1.9	4.9	3.6	10.2	3.7	10.0	6.2	4.8
c changed responses (2) ..	7.1	2.4	1.2	4.9	1.4	4.1	.6	2.3	3.8	9.2	6.1	6.2	4.7	4.3

The main trends of this table are of course those of the one for the whole body of responses; but it is interesting to observe the great disparity in the usualness of the repeated and changed responses. The usual responses are those which are well grounded in experience, and naturally have the greatest chance of reappearance. On the other hand, there is a certain irregularity about it; the repeated responses of IV. are the most usual of all, the changed ones are in both experiments less usual than those of I., X. or XII. Rather unusual repeated responses but relatively usual changed responses are given by XIII. and XIV. The anomaly of Case V. is again evident. The correlations are not so well marked in the changed responses as in the series as a whole; the personal variation is 1.3 as against group variations of 2.8 and 1.9. The orders have 29 per cent. of displacement, 20 per cent. excluding V.; there are 80 per cent. of plus signs from the median.

In concluding the immediate discussion of the type in the community of response, it may be remarked that the distribution of the single figures from which the median is calculated uniformly have a distribution markedly skewed toward the 'individual' end, with now and then suggestions of a species at frequencies between 30 and 40 per cent. A few of the records were examined, by per cent. of like signs, with reference to correlations between the increased individuality and long reaction time; a positive relationship appeared,

rather constant, but quite slight. The median of community in all reactions of over 50 (10 seconds) in the first experiment was, however, given as no higher than 3.5 per cent.

Individual differences in community of response, however well defined or clearly significant, are subject to considerable limits in their application. One cannot operate with this measure ouside the stimulus words from which frequency tables are compiled. It is certainly most desirable to investigate if there exists a method of attaining a similar insight into free association material that shall not be subject to these limitations. The obvious direction of such an inquiry is towards the superficial relation of the response word to the stimulus word. It has been put forward by previous investigators that certain psychological attitudes[1] are dynamically related to special relationships of this nature in the association test. The present task is then to test the value of this principle in the determination of individual differences in response to the test.

The relationships to be considered have already been described. There are here five categories, (1) the *predicate*, (2) the supraordinate, (3) the contrast, (4) the miscellaneous or 'internal-objective,' and (5) the speech-habit group.[2] Every individual's associations fall, in varying proportion, into these five classes. Continuing to deal with the Kent-Rosanoff experiment, our first question, following the previous system, is how the personal variation in these categories compares with the group variation.

It will be remembered that there are two experiments with fourteen subjects, fourteen months apart. The averages, M.V's. between the different subjects, and M.V's. between the same subjects, are as follows:

The figures speak for themselves about as well as any discussion might do. Save only in the speech-habit category, the individual differences are as clear cut as in the objectively determined frequencies of response (it may be

[1] *E. g.*, the "Prädikattypus" of Jung.

[2] PSYCHOLOGICAL REVIEW, Vol. XVIII., pp. 229-233. For further definition, illustration and discussion of these five categories as such, the reader is referred to this paper.

Category	Group Av.		Group M.V.		Personal M.V. 1st and 2d Exps.
	1st Exp.	2d Exp.	1st Exp.	2d Exp.	
Predicate	19.5	22.9	13.5	13.8	3.7
Supraordinate	15.5	13.5	9.4	12.5	3.21
Contrast	14.9	12.1	8.1	7.1	3.2
Miscellaneous	44.5	47.6	5.9	6.9	2.2
Speech-habit	5.5	3.9	1.6	1.3	1.6

remarked in this connection that some eight months intervened between the classification of the two sets of results). They are quite definite in the predicate, for external reasons the most important, category, and somewhat less so in the supraordinate and contrast groups. The order in the number of predicates shows 9 per cent. of displacement between the two experiments. The Pearsons in the different categories in the two experiments are as follows:

Predicate.....83
Supraordinate.....77
Contrast.....76
Miscellaneous.....84
Speech-habit.....03

Relative tendency towards one category strongly implies tendency towards it later, save only in the small category of speech-habit responses. Definite fidelity to type appears, then, to be characteristic of the subjects in these functions of the experiment.

While these are the main findings in this respect, there is another feature of the variability that is not without interest. Each subject naturally varies a certain amount from the group in each experiment, and then a certain amount from himself between the two experiments; it is the average of these figures for each category that has just been quoted. It were also a significant question of whether a subject who tended to vary much from the group also tended to preserve greater or less fidelity to his own type. A distinct negative correlation, 14 per cent. of + signs from the median, − .40 Pearson,[1] is indicated between these two functions. This is probably significant mainly for the subjects who lie

[1] As with the median, only two cases are positive, but these pronouncedly so.

at some distance from the group average, and tend to preserve their type. At first glance, it would scarcely seem reasonable to suppose that a subject who was close to the group average, should vary more from himself than one who was far from it; yet the function does seem to possess some properties of this nature, as will be brought out in the observations of a more intensive nature now to be described.

Hitherto, no comparisons of the material with reference to association types have been made between series of different stimulus words. The extent of the error which different series of words might introduce was not known, and it was therefore endeavored to avoid it, though it was reasonably certain that its extent was not very great. A fairly precise idea of it, however, may be attained in comparing the relation of the associative categories in the first half of the Kent-Rosanoff experiment to those in the second half;[1] there thus being two groups of fifty words each. Changes occurring here are presumably due rather to a difference in the stimulus words than to fluctuations of experimental attitude such as might operate in experiments on different days. The following figures, then, give the mean personal variation of the five categories for the first and second halves for both experiments:

	Pred.	Supra.	Cont.	Misc.	Sp. H.
1st exp.....	1.2	1.3	2.0	1.8	1.4
2d exp.....	2.0	1.8	1.4	2.8	.6

The figures of the personal variation between the two halves of the same experiment are somewhat, not greatly, smaller than between the two experiments themselves; though considering that it is for only half the cases, they can scarcely be counted smaller at all. It is peculiar that the second experiment should show greater variability in this respect than in the first; the contrary if anything would be expected.

The indication, however, is that the variation between the different stimulus words would not be sufficient to obscure

[1] A better procedure would have been to compare alternate words.

real individual differences in association type, even though the stimulus words furnishing the material were not the same for each subject.

This question is more searchingly tested in the subsequent analysis of the 7,000 reactions which form the material of the already reported practice experiments. While this material is twice as extensive as that from the Kent-Rosanoff experiment, its interpretation with reference to the matter of types depends largely on whether or not the daily changing of the stimulus words induces special changes in associative trend peculiar to the series that is used. In any case, whatever alteration of type the changes in the stimulus words produces it ought to be in the same direction for each of the seven subjects, since the changes in the stimulus words are the same for each one; that is, if an association series did exist which prejudiced in favor of the egocentric or other categories of reaction, there ought to be a uniform change in that direction among the subjects; indeed, that would be our only criterion of such a change. It has actually been found that on nearly half the days when the egocentric category shows deviations greater than the M.V., such greater variations are in different directions for different subjects. While some influence probably is exercised by particular series of words, it is not believed sufficient, in view of such like findings, to offset the validity of the subsequent interpretations.

A priori such an influence would here be quite improbable, since the order of 1,000 words is a quite random selection.

According to observed deficiencies in the conception of the predicate category obtaining in the above it is here widened and the name changed to the *egocentric*. This widening is mainly at the expense of the miscellaneous and speech-habit categories, and consists chiefly in the transference to the former predicate category of reactions where a proper name is involved. This adds much to the significance of the classification.

As there are 1,000 reactions for six, and 500 for two subjects, it is possible to consider the prevalence of special categories in the direct comparison of the various subjects.

The averages and mean variations of each of the eight subjects in the five categories are as follows:[1]

Subject	Black	Brown	Red	Orange	Green	Blue	VII	VIII
Egocentric	8.5	7.6	8.7	21.3	22.1	10.7	8.7	16.7
M.V.	3.2	2.6	2.8	3.2	2.6	3.6	3.5	1.7
Supraordinate	1.4	3.3	6.1	2.5	5.0	3.4	6.8	1.7
M.V.	1.0	1.7	3.2	2.2	2.0	1.8	2.1	.9
Contrast	5.0	5.1	3.1	.9	.5	1.9	4.0	.6
M.V.	2.1	2.4	1.8	1.1	.5	2.1	2.0	.6
Miscellaneous	27.7	28.3	28.8	20.3	20.2	26.9	26.3	28.0
M.V.	4.2	2.6	4.2	3.3	2.5	3.8	2.8	2.6
Speech-habit	6.8	5.2	3.0	5.0	2.0	5.2	3.9	3.0
M.V.	1.9	2.1	1.2	3.4	1.5	1.7	1.3	1.6

It appears here, as also in the previous analysis, that while there is an influence of practise on the form of association, it does not have any special influence on the type, save only in subject BLUE.

In the common acceptation of the experiment, the essential significance lies in the relations of the egocentric and the miscellaneous categories, and more especially in the character of the egocentric category. It is very evident that in this respect three steps are represented in the results: (1) those showing the fewest egocentric reactions, namely, BLACK, BROWN, RED, BLUE and VII.; (2) subject VIII., who occupies a midway position; and (3) ORANGE and GREEN, who occupy positions quite high in the scale of egocentricity, not only here but in the frequency valuations of the Kent-Rosanoff experiment where they are, respectively, subjects VII. and II. These three steps are separated from each other well beyond the limits of the probable error; save in one case only, beyond that of the mean variation. The variability of the measures in each subject is so small compared to the total range in the egocentric category, that there can be little doubt that the averages represent definite points of the scale about which each subject's results center with comparatively little chance fluctuation. On the basis of

[1] These are the actual figures from series of fifty stimulus words; they must be multiplied by two for direct comparison with tables on page .

these figures then, we may assign definite and limited positions to the different subjects along the scale of egocentricity in response. In the other categories, individual difference is much less pronounced. One may note the relative abundance of supraordinate reactions in VII., and their absence in BLACK and VIII.; also the peculiar absence of contrast reactions in ORANGE, GREEN and VIII. The excess of egocentric reactions in ORANGE and GREEN is associated with a corresponding decrease in the miscellaneous category. Elsewhere, little individual difference exists between these subjects, the comparative table of personal and group variations being:

	Egoc.	Supra.	Cont.	Misc.	Sp. H.
Average group var.....	5.1	1.9	1.7	2.7	1.3
Average personal var.....	2.9	1.9	1.5	3.3	1.8

The main interest of further calculation attaches to the possible correlations of the various categories. Thus there is a pronounced negative correlation, approximating −.65 Pearson between the supraordinates of the first Kent-Rosanoff experiment with twenty-five subjects, and the contrast associations of this group. A somewhat less marked negative relationship, −.45, exists between the contrasts and the predicates in this experiment. In the table just quoted, this is markedly changed. The *egocentric* category, which is supposed to represent more accurately the same psychological attitude that the *predicate* implies, shows an extreme negative correlation with the contrasts, −.95. On the other hand, the supraordinates are so much reduced by practise effect that the initial negative relationship with the contrast has disappeared, and given place to a slightly positive one of .14. The results are not inconsistent when one takes into account the practise effect on the supraordinates and the changed conception of the egocentric. A possible interpretation is that the mental mechanism of the contrast association is throughout in marked opposition to that of the predicate or egocentric. At the beginning of practise this is true also of the supraordinates, which here result largely

from the adoption of a conscious 'set' and are thus egocentrically determined; the disappearance of the supraordinates with practise coincides with the increasing freedom of response, and diminished necessity for conscious 'sets.'

A previous paragraph on the Kent-Rosanoff material brought out the result that there were marked individual differences in fidelity to type, and that those tended to be more faithful to their own types who were further away from the group averages. The practise experiments throw the essential features of this result into stronger relief. The 'average' association type appears to be a distinctly concrete one. A fair average association record of fifty words would contain about 10 predicates, 7 supraordinates, 8 contrasts, 22 miscellaneous and 3 speech-habit. The great departures from this type are in the egocentric direction; and the above finding may be re-stated in terms of a greater fidelity to type among individuals of markedly egocentric reaction tendency. It will be seen that the number of egocentric reactions is not so variable in ORANGE, GREEN and VIII., who represent egocentric types, as it is in BLACK, BROWN, RED, BLUE and VII., who represent concrete types.

The relatively large M.V. of the egocentric category in ORANGE is due largely to three days, on which there was an extraordinary variation from type; the median variation would be 2.6. Larger single variations from type in this category occur among the ecogentric subjects than among the concrete; they tend to keep closer to type, but may be thrown farther away from it.

It will be distinctly understood that by an association type is meant the tendency of an individual to preserve a definite associational trend and this only; nothing has been adduced to show whether there are distinct *species* of 'egocentric' 'concrete,' or perhaps of other types. The number of cases is scarcely sufficient for a final determination of whether species exist, but they are not indicated in the examination that it has been possible to make. The question of species is probably to be cleared up, however, by a study to this end of the material gathered by Kent and Rosanoff, *in extenso.*

From actual observation, as well as from what we know of the psychology of the predicate reaction, there should be every reason to expect definite negative correlation between the tendency to give the common responses of the Kent-Rosanoff tables and the tendency towards an egocentric reaction type as given in the number of predicate responses. One can indeed see that 'unusual,' and 'egocentric' reactions are the product of largely the same mental mechanisms. The actual figures of this correlation are:

Median of Community and Number of Predicates	
1st exp.	−.74
2d exp.	−.74

This material was classified before the conception of the egocentric category had replaced that of the predicate, it was this very classification that indicated the desirability of the 'egocentric' conception. The *r*'s would be materially higher if the 'egocentric' category had been employed, for it would have included a fairly numerous group almost always summed up in the 'individual' reactions, the proper names.

The figures of the correlation are the empirical measure of the validity of this conception. The object has been to define a category which shall be egocentric in fact as well as in name, and these coefficients plus the considerable margin of proper names, give the extent to which this object has been reached.

The results which appear more significant for further progress with the test may be summarized as follows:

1. The previously reported differences in the association time of various subjects are fairly inherent; that is, a certain range of reaction time seems characteristic of a given individual. A long median reaction time is usually associated with a skew distribution much spread out at the long end. Such an abundance of very long reaction times indicates the presence of special difficulties in responding. These may be of widely differing causations, the determination of which demands further analysis of the record.

2. The number of individual reactions appears hardly suitable for the comparison of single records and the median

value of the 100 reactions is substituted. In the tendency to give common or specialized responses by the Kent-Rosanoff experiment, individual differences are much more clear cut than in the association time. The extreme range of individual difference in single records is about 20 : 1 as against 4 : 1 in the association time. The responses (of the same subject to the same stimulus word) which differ in the two experiments, are in general less than one third as 'usual' responses as those which are the same on each occasion.

3. In this same material of the Kent-Rosanoff experiment, definite fidelity to type appears in the tendencies to give predicate, supraordinate, contrast and 'internal-objective' responses, but not in speech-habit reactions. A relatively greater fidelity to type is found among individuals whose reaction type is further distant from the central tendency of the group.

4. When the number of stimulus words in a chance-selected series is as great as fifty, the results of different series of stimulus words are for practical purposes comparable with each other in respect to the above classification by logical category. A study in eight subjects, through extended series of stimulus words, illustrated in greater detail the distinction between the individual of the *egocentric*, as apart from the individual with the *concrete* association type. Of the eight subjects, two were pronouncedly egocentric, one rather so, and five of the more concrete type.

5. There is, initially, marked negative correlation of the contrast category on the one hand, with the predicate and supraordinate categories on the other. Practise breaks down this correlation for the supraordinates, but preserves it for the mechanism represented by the predicates. Pronounced negative correlation is indicated between the egocentric (resp. predicate) category and the community of response; that is, 'egocentric' responses are seen to be unusual in the Kent-Rosanoff tables, as one would expect.

There is little significant correlation between the usualness of the response and its reaction time, as was also observed by Kent and Rosanoff; it is possible that a more definite, though

rather complex, relationship obtains in this respect between different individuals or groups. The fidelity to association type has also been largely stated in terms of correlation measures, which range between 73 and 86 per cent. positive.

It has thus been attempted to indicate the character of the variation in the principal functions of the association test; the causes of these variations, under what conditions they occur, and their relations to the personality, are to be discussed elsewhere. The present result is that the findings of quantitative study have not contradicted the data of ordinary observation, and give an affirmative answer to the question of association types.

EXPERIMENTAL STUDIES OF RHYTHM AND TIME

BY J. E. WALLACE WALLIN

III.[1] The Estimation of the Mid-Rate between two Tempos

A. Explanation of Problem

The problem of this investigation consists in the selection or production of a tempo lying midway between two standard tempos produced by a Verdin metronome. For example: suppose the subject is made to listen to the 40 (40 per minute) rate, and then, after a brief interval, to the 208 rate. What is the mid-tempo between these limiting stimuli? How accurately can the subjects make a mental estimation of the mid-rate? The problem, of course, does not concern the mathematical computation of the mid-rate —none of the subjects knew the rate of the standards[2] employed—but the selection from among a number of variables of the rate which appears to lie midway between the standards, or the production by tapping of the mid-tempo.

Three pairs of standards were employed in the following combinations: 40–208; 208–40; 72–176; 176–72; 104–144; and 144–104. The mathematical mean is the same for all three pairs, namely 124 beats per minute, or 2.066 beats per second. Accordingly the length of the mid-interval (the time from click to click) is .483 sec.[3]

[1] Articles I. and II. appeared in the March and May numbers, 1911, of the Psychological Review.

[2] The extreme or limiting rates will be referred to as the standards or rates.

[3] The computation of the arithmetical mean is based on the assumption that if the difference between r_2 and r_1 is equal to the difference between r_1 and r (see accompanying diagram), then r_1 = the arithmethical mean between r and r_2. On the other hand, if the relative sensible discrimination is constant the difference between r_2 and r_1 will be greater than the difference between r_1 and r when the two seem equal to sensation, whence the proportion $r : r_1 = r : r_2$., or $r_1 = \sqrt{r \cdot r_2}$. This gives the geometrical mean, which amounts to the following for the different pairs: 91.2 for 40–208; 112.6 for 72–176; and 122.3 for 104–144. (The corresponding geometrical means in interval length are .65 sec., .53 sec. and .49 sec.)

RATES PER MINUTE

r_2	r_1	r
Faster standard	Mid-rate	Slower standard
208	124	40
176		72
144		104

(208 + 40 = 248 ÷ 2 = 124. 208 − 40 = 168 ÷ 2 = 84. 84 + 40 = 124. 208 − 84 = 124.)

The attempt was made to vary regularly the time order in which the different standards and the different pairs of combinations were given to the different subjects, in order to neutralize any constant errors in the final averages. Thus to some the 40-208 pair was given first; to others the 72–176, and to others the 104–144 pair. Likewise to some subjects the faster of the two standards was presented first, and to others the slower. Thus some listened first to the combination 208–40, and then, after an interval of four or five seconds, to the reverse order, 40–208; while for the others the order was, first, slow-fast (40–208), and second, fast-slow[1] (208–40).

The precise order is given in detail for each subject in the columns in the tables headed 'Time Order.' Thus, to subject L., Tables VI. to IX., the first combination presented was 40–208 (indicated by I. in the second column); then followed in order 208–40 (II.), 176–72, 72–176, 144–104 and finally 104–144. In all cases the results are averaged separately for the rapid-slow and slow-rapid combinations of standards. Thus Table I. averages the results for 40–208, and Table II. for 208–40. The last average in II. is the average for both tables. In Tables VII. to IX. not only are the separate and combined averages given in the same table, but the results are arranged in subordinate groups according as the rapid-slow combination was given *before* or *after* the slow-rapid. This order is indicated by the letters *a* and *b* in the third column of these tables, *a* indicating that the given combination of standards was given first and *b* that it was given second. Thus in Table VII. the order 40–208 was given before the order 208–40 to J, H and D, while 208–40 was given before 40–208 to W, S, R, B and Ms.

Invariably the two tests for the same pair of standards were finished before either of the other two pairs were presented. Thus 40–208 was always directly followed by 208–40 (or vice versa), and never by one of the other two pairs.

The first experiment was conducted in the Princeton Psychological Laboratory during February, March and April, and the second during May and June in 1906—the publication has been delayed for reasons given in the first article. The subjects consisted of seniors, all of whom had taken two terms of work in the laboratory, and Professors E. G. Spaulding, W. T. Marvin, M. P. Mason, A. L. Jones

[1] In discussing the various pairs the slower standard will be mentioned first (40–208, 72–176, 104–144). But, unless otherwise indicated by the context, the facts stated are based on the averages for both the slow-fast and fast-slow series.

and H. C. Warren. The latter two and three of the students (D, H, R) served in both experiments.

The subjects invariably sat with their backs towards the apparatus, hence could not see the manipulation of the metronome, and were given no information other than that involved in the explanation of the problem.

B. First Experiment[1]

Estimation of the Mid-rate between Two Metronome Tempos, by the Method of Choosing a Variable in a Serial Procedure

Table I

Standards: 40 and 208 (beats per minute)

Starting with Variable too Slow

S.	Time Order	Variable Started at	Beats per Minute	M.V.
L.	II.	58	92	
K.	III.	63	116	
H.	II.	69	92	
R.	IX.	76	104	
Wa.	XII.	69	126	
D.	XII.	66	88	
A.	V.	60	84	
J.	VIII.	80	116	
G.	VI.	100	112	
We.	I.	69	112	
Ms.	III.	72	132	
I.	IV.	88	116	
Average[1]			107.5	12.9

Starting with Variable too Fast

L.	I.	160	88	
K.	IV.	138	108	
H.	I.	138	126	
R.	X.	168	132	
Wa.	XI.	138	120	
D.	XI.	138	116	
A.	VI.	138	124	
J.	VII.	160	138	
G.	V.	138	120	
We.	II.	138	120	
Ms.	IV.	152	126	
I.	III.	138	120	
Average[1]			119.8	7.9
Average[2]			113.6	10.4

Explanations follow Table VI.

[1] Being beyond the reach of reference books at the time of compilation I am obliged to lay out the data purely empirically. Moreover, other lines of work now so engross my attention that no other course is possible.

TABLE II

Standards: 208 and 40

Starting with Variable too Slow

S.	Time Order	Variable Started at	Beats per Minute	M.V.
L.	IV.	58	92	
K.	I.	63	100	
H.	III.	60	92	
R.	XII.	76	96	
Wa.	X.	56	80	
D.	IX.	60	82	
A.	VIII.	72	84	
J.	V.	48	66	
G.	VII.	54	76	
We.	IV.	100	112	
Ms.	II.	66	76	
I.	I.	69	120	
Average			89.6	12.3

Starting with Variable too Fast

L.	III.	160	96	
K.	II.	144	124	
H.	IV.	138	116	
R.	XI.	160	120	
Wa.	IX.	138	96	
D.	X.	138	126	
A.	VII.	132	108	
J.	VI.	138	104	
G.	VIII.	132	112	
We.	III.	152	120	
Ms.	I.	144	88	
I.	II.	138	124	
Average[1]			111.1	10.6
Average[2]			100.4	11.4
Average[3]			107.0	10.9

Explanations follow Table VI.

TABLE III

Standards: 72 and 176

Starting with Variable too Slow

S.	Time Order	Variable Started at	Beats per Minute	M.V.
L.	V.	88	112	
K.	VII.	96	120	
H.	VIII.	96	126	
R.	II.	92	108	
Wa.	II.	96	116	
D.	III.	100	116	
A.	XII.	108	116	
J.	IX.	100	116	
We.	VIII.	108	112	
Ms.	V.	96	112	
I.	V.	108	126	
Average[1]			116.3	4.1

Starting with Variable too Fast

L.	VI.	152	112	
K.	VIII.	138	120	
H.	VII.	144	126	
R.	I.	138	124	
Wa.	I.	144	122	
D.	IV.	152	132	
A.	XI.	132	116	
J.	X.	138	126	
We.	VII.	132	124	
Ms.	VI.	132	124	
I.	VI.	132	126	
Average[1]			123.0	4.0
Average[2]			119.6	4.0

Explanations follow Table VI.

TABLE IV

Standards: 176 and 72

Starting with Variable too Slow

S.	Time Order	Variable Started at	Beats per Minute	M.V.
L.	VII.	88	102	
K.	VI.	92	120	
H.	VI.	92	116	
R.	III.	92	112	
Wa.	IV.	104	116	
D.	II.	96	108	
A.	X.	100	112	
J.	XI.	104	114	
We.	VI.	104	116	
Ms.	VII.	80	104	
I.	VIII.	112	120	
Average[1]			112.7	4.6

Starting with Variable too Fast

L.	VIII.	152	112	
K.	V.	138	120	
H.	V.	138	126	
R.	IV.	138	124	
Wa.	III.	138	116	
D.	I.	144	126	
A.	X.	138	116	
J.	XII.	138	126	
We.	V.	132	124	
Ms.	VIII.	132	126	
I.	VII.	138	126	
Average[1]			122.0	4.3
Average[2]			117.3	4.4
Average[3]			118.4	4.2

Explanations follow Table VI.

TABLE V

Standards: 104 and 144

Starting with Variable too Slow

S.	Time Order	Variable Started at	Beats per Minute	M.V.
L.	XI.	108	126	
K.	X.	108	124	
H.	IX.	108	124	
R.	VII.	112	120	
Wa.	VII.	112	122	
D.	VI.	108	120	
A.	III.	112	120	
J.	II.	112	120	
G.	IV.	112	120	
We.	IX.	112	120	
Ms.	XI.	116	124	
I.	X.	112	120	
Average[1]			121.6	1.9

Starting with Variable too Fast

L.	XII.	138	120	
K.	IX.	132	120	
H.	X.	138	124	
R.	VIII.	138	118	
Wa.	VIII.	132	126	
D.	V.	138	126	
A.	IV.	138	126	
J.	I.	138	120	
G.	III.	132	116	
We.	X.	138	126	
Ms.	XII.	138	126	
I.	IX.	132	120	
Average[1]			122.3	3.3
Average[2]			121.9	2.6

TABLE VI

Standards: 144 and 104

Starting with Variable too Slow

S.	Time Order	Variable Started at	Beats per Minute	M.V.
L.	X.	108	124	
K.	XI.	108	120	
H.	XII.	112	124	
R.	V.	108	124	
Wa.	VI.	108	122	
D.	VIII.	112	120	
A.	II.	108	124	
J.	III.	108	120	
G.	I.	108	124	
We.	XII.	116	120	
Ms.	IX.	112	122	
I.	XII.	112	120	
Average[1]			122.0	1.6

Starting with Variable too Fast

L.	IX.	138	120	
K.	XII.	132	116	
H.	XI.	138	124	
R.	VI.	132	120	
Wa.	V.	138	120	
D.	VII.	132	124	
A.	I.	138	124	
J.	IV.	132	117	
G.	II.	138	122	
We.	XI.	132	126	
Ms.	X.	132	124	
I.	XI.	138	126	
Average[1]			122.0	2.7
Average[2]			122.0	2.1
Average[3]			121.9	2.3

Explanation of Tables.—Unit of measurement: the number of clicks made by the metronome per minute. S.: subject (all males). Time order: order of the temporal succession in which the various combinations were given, from I. (first) to XII. (last) M.V.: mean variation between the counts given in fourth column of the tables. Ave.[1]: separate averages for the group of measurements in which the variable started either too slow or too fast. Ave.[2]: separate average for the slow-fast or fast-slow combinations of a given pair, the average of the two Ave.[1]'s. Ave.[3]: general average for a given pair, the average of the Ave.[2]'s. The standards were given in the order indicated in each table. Thus 40–208 indicates that the 40 rate (40 beats per minute) was given before the 208.

The Selection of the Mid-rate in a Serial Procedure with Variable Rates Introduced between the Limiting Stimuli (Method of Impression, or Method of Mean Gradations).

The instructions given to the subjects were substantially as follows: "I shall have the metronome click off a series of beats which we shall call the *first standard.* Then I shall click off a series of slower beats (or faster in case the first series consisted of the slow standard) which we shall call the *variable* series or beats.[1] Finally I shall click off a series at a still slower rate of speed which we shall refer to as the *second standard.* I want you to listen carefully (closing your eyes, if necessary) to these three series and tell me, after all three have been given, whether the speed of the variable beats seemed to be nearer the speed in the first standard, or nearer the rate in the second standard." (Or, 'whether the variable

[1] In a few preliminary trials the variable was sometimes given as the second series, and sometimes as the third. Some subjects preferred the first arrangement and some the other.

was too slow or too fast'; or 'whether the length of the intervals in the variable series was midway between the length of the intervals in the first and the second standards.') If the subjects responded correctly that the variable beats were too fast it was explained that in the next test the speed of the variables would be reduced and that this process of minimal approachments would be continued until the variable was judged midway between the extremes. After the equality judgment had been rendered the same step was repeated once or twice, and in case the judgment was reversed the steps were continued until an equality judgment was again obtained. (All except 'I' changed their equality judgments in an average of 2.1 of the twelve series.) After a satisfactory equality judgment had been obtained, as above, the step-procedure was continued in the same direction for 10 of the subjects until the variable was again judged unequal (too slow in the above case). (The equality range averaged 9.1 beats for the 40–208 pair, 4.4 beats for 76.176 and 3.0 beats for the 104–144.) The mid-tempos recorded in the tables are invariably the *first equality judgments* rendered which were not later changed.

After the mid-rate had been thus selected, with the variable starting too fast, the reverse procedure was followed (variable too *slow*, minimal changes). If an equality judgment was given at the very start the variable was started still slower.

It is thus evident that the subjects knew the direction of the progressive changes in the variables series. It was explained, however, that the steps might vary somewhat, and that they would not always start at the same rate of speed in the different combinations. It was necessary to make this precautionary statement in view of criticisms raised against the serial step procedure,[1] and in order to counteract the possible tendency of subjects to count the number of steps employed in a given combination and utilize the knowledge of the number of changes as a guide for judging

[1] Angell states that he was able to get any midpoint of sound intensities he wanted by the serial method, the midpoint varying with the initial value of the variable and with the size of the steps.

later combinations.[1] The rate at which the variable was started for any subject can be determined by consulting column three in Tables I to VI. The steps usually employed were those provided on the pendulum of the metronome, but near the estimated mid-point they were sometimes halved, as explained in the second article. They were always halved when one of the adjacent speeds was judged too fast and the other too slow.

In each of the three series (first standard, variable and second standard) the metronome was allowed to click about ten times—sometimes a little less, sometimes a little more. The one series followed the other as rapidly as the weight on the pendulum could be properly adjusted—an interval of three or four seconds. The number of clicks in each series and the length of the silence between the series seemed to be satisfactory to all the subjects, except one who sometimes found the pause too long (Ms), another who sometimes found it slightly too short (I), and two who found the sound series at times too long (Ms and L, the latter referring to the 40 rate).

The sliding of the weight on the pendulum produced a slight noise, of longer or shorter duration according to the extent of the displacement. Eight who noticed the noise reported that it was not utilized as a secondary criterion for estimating the mid-point, although two found that it distracted the attention and that they had to use deliberate effort to disregard it (Wa, D).

Two or three sittings (three for A, D, I, J, Wa, We) were required to complete the experiment. During the first sitting only one pair of standards was used. Likewise one pair was used in each of the second and third sittings for those who sat three times. The sittings varied from 40 minutes to one hour.

Eight subjects found that the experiment was more or less fatiguing, particularly toward the end of the sitting, and most frequently in connection with the 40–208 pair. One, in order to 'keep the rates alive,' had to resort to nodding the head. Contrariwise, eight did not find the work very fatiguing, particularly with the 104–144 pair. The standards farthest apart accordingly occasioned the greatest strain on the attention.

Of six subjects interrogated all considered the problem considerably more difficult than the parallel problem of choosing a line equally different from a short and a long line. Likewise of four questioned all found the experiment harder than an earlier experiment (to be described in the next article) on determining the maximum number of categories into which the metronome series of auditory rates could be grouped. The difficulty most frequently experienced was to conceive, imagine or know where to locate the mid-rate (mentioned by seven). Five were able to 'ideate' the mid-rate,

[1] Only one, however, reported that he tried to use this expedient once (I).

or to carry in mind a temporal standard (ideal beats or ticks) corresponding to the mid-rate, one obtained a mental criterion after a few trials, and one felt that his criterion changed in the pair with the slowest standard. Five subjects, on the other hand, did not use any mental standard, or any fixed mental beats. Another frequent difficulty was that of keeping the standards in mind (by six)—more difficult with the first standard (by five) than with the second standard (one), or the variable (one). Agreeably with these judgments, the first standard required more attention (reported by five) than the last. Two gave more attention to the variable; one, to the two standards than the variable; one, to the first standard and the variable than to the second standard; one, equal attention to the different series; and one gave attention mainly to the 'contrast' between the rates. Two paid most attention to the slow standard, and one to the fast. The slower standard was retained in the memory more easily than the fast (by six as against two subjects). Although the introspections of the subjects vary more or less from time to time or series to series,[1] it is patent that the *first* standard and the *fast* standard are the most difficult to remember, and would for this reason necessarily require more attention.

The standards *nearest together* (104–144) were felt to be the most *difficult*, by six, (because of the small contrast between them, or the difficulty of distinguishing the grades of difference, or retaining the standards); and the standards *farthest apart* (40–208), by five (because one rate was too fast and the other too slow, or the slow could not be grasped as a unity, or the equating became uncertain, or the variable could not be compared with the slow rate). Three regarded the middle standards (72–176) as the *easiest*, two the nearest together, one the farthest apart; while one considered the difficulty about the same for all three pairs, and one for the two less widely separated pairs. Altogether, introspectively the difficulty is the greatest for the two pairs which are the nearest and the farthest apart.

Less frequently enumerated difficulties encountered in the experiment were the acquisition of a 'habit speed' for the mid-rate in one combination which influenced the selection in other combinations; the reversing of the order of giving the standards, which caused the subject to lose his 'mental mid-point'; the occasional irregularity of the 208 beats; and the lack of a good ear for time.

Results.—1. The selected mid-rates are invariably *less (slower) than the arithmetical mean.* The error amounts to 2.1 beats per minute for the 104–144 pair, 5.5 beats for the 72–176 pair, and 17 beats for the 40–208 pair. For the first two pairs the error is practically negligible; the average estimations are very accurate.

It is observable that the estimated mid-rates are slightly nearer the geometrical than the arithmetical mean for the two pairs 40–208 (the divergence from the geometrical mean being 15.8 beats as against a divergence of 17 beats from the arithmetical mean) and 104–144 (0.4 as against 2.1), but

[1] This is true for many of the subjects on most of the questions introspected. The discrepancies in the reports are due to the differences between the standards, differences dependent upon the time order, and other undetermined causes.

the reverse is the case with the 72–176 pair (5.8 as against 5.6).

2. Both the absolute and relative *inaccuracy of estimation increases with the increase in the distance between the limiting stimuli.* The absolute error for the 72–176 pair is two and one half times as large, and for the 40–208 pair eight times as large, as for the 104–144 pair. The relative errors—that is, the amount of error expressed as a per cent. of the distance between the limiting stimuli or standards—amount to 5.0 per cent. for the 104–144, 5.3 per cent. for the 72–176, and 10.1 per cent. for the 40–208 pair. Thus the relative errors for the two pairs in which the standards are the closest is about the same, while the relative error for 40–208 is twice as large as for either of the other two pairs.

These results are confirmed by the absolute M.V's (M.V. column), which amount to 10.9 beats per minute for 40–208, 4.2 beats for 72–176, and 2.3 beats for 104–144. Relative to the distance between the limiting stimuli, however, the corresponding mean variations (relative M.V's) amount to 6 per cent., 4 per cent. and 5.7 per cent., the relative M.V. thus being least for the 72–176 pair.

Likewise, the absolute range of equality (the range within which equality judgments were rendered), which furnishes an indirect measure of uncertainty, is the smallest for 104–144 and the largest for 40–208, but relative to the distance between the standards the range is smallest for 72–176 (4.2 per cent.), followed by 40–208 (5.4 per cent.) and 104–144 (7 per cent.).

These experimental demonstrations, particularly that the 40–208 pair is the most inaccurate, are supported by the instrospections. The 40–208 pair occasioned the greatest amount of *fatigue* and the 104–144 the least; six subjects reported that their estimations were *uncertain* with the 40–208 pair, but only two with the 104–144 pair, and two with the 72–176 pair; while the estimations were considered *most* certain with 40–208 by one, and with 72–176 and 104–144 each by four subjects (different subjects, of course, in each pair), and intermediate for 72–176 and also 104–144 by four. One regarded the latter two sets as about equal. All felt *least* certain with the 40–208 pair.[1]

3. *Giving the slow standards first* enabled the subjects to make *more accurate* estimations than giving the fast standards first, although the differences are small, amounting to 6.6 beats for 40–208, 1.1 for 72–176, and 0.1 for 104–144 (but in the last the accuracy was greater with the fast standard first), or an average of 2.6 beats for the three standards.

[1] Why more subjects should regard the pair of standards nearest together as more 'difficult' than the pair farthest apart, as we saw earlier, although the certainty of the equating was the least for the latter, is not clear. The two judgments appear to be contradictory.

This conclusion gains slight support from the *introspections*. Six preferred to have the *slow* standard come first (because it was easier retained, or the decisions could be rendered faster, or the comparison was easier, or it seemed like the natural order); while five preferred the *faster* standard first (because it was easier retained, or because, although harder to retain, it could be kept more easily with the finger, or because the decisions could be made faster, since the slow standard was 'fixed in the mind.' Six, however, at one time or another, felt that the order was immaterial. The only effect material to one was the feeling of uncertainty occasioned by *reversing* the order of the standards.

4. The estimations are rendered *more accurately* when the *variable is started too fast* rather than too slow (slower than the mid-tempo, of course). The differences in beats per minute are as follows for the various combinations: 12.3 for 40–208; 10.8 for 208–40; 3.3 for 72–176; 4.6 for 176–72; .3 for 104–144; and .00 for 104–144. The average difference for all combinations amounts to 5.2 beats.

The conclusion is confirmed by the M.V's, which are smaller in all the combinations in which the variable was started too fast except two (104–144 and 144–104). The average difference for all combinations amounts to .8 beat.

On this point, however, the introspections are neutral. Five preferred to have the variable start *too fast* (because it was easier to retain, or the mid-point could be reached faster, or since the slower standard was easily retained it was only necessary to compare the two faster rates, which was easily done since they came nearer together); five, at one time or another, preferred to have it start *too slow* (because easier to retain, or the judgments were more certain, or it was easier to compare the two slower series of rates); while six expressed no preference (twice for 40–208, thrice for 72–176, and five times for 104–144, so that the feeling of neutrality mostly attached to the standards closest together). There is a tendency to express preference when the standards are rather far apart.

5. The *degree of accuracy* with which different subjects are able to *determine the mid-point* between different limiting rates of temporal successions differs considerably. This has been indicated by the M.V's, and may be seen from a statement of the largest and smallest errors made in each pair. The largest errors in the 40–208 pair was 66 beats; in the 72–176, 22 beats; and in the 104–144 8 beats. Contrariwise, there were three instances in which the estimations were absolutely correct in the first of the above pairs, five in the second, and thirteen in the third.

This experimental difference is paralleled by a corresponding subjective difference in certainty. Nine in one combination or another felt uncertain, or rated their estimations as guesses, particularly near the point of equality; four, in one combination or another, rated them as better than guesses; while eight felt certain or fairly certain

in one combination or another. One felt more certain of his 'just different' estimations than his equality judgments. The subjective certainty, as we saw above, was least for 40–208.

Since the subjective certainty varied more or less in the different combinations, the question naturally arises whether the feeling of certainty is paralleled by a corresponding superiority in the experimental records. In the case of nine subjects the records do (some strongly, others only partly), while in the case of four they do not, corroborate, or only corroborate in some combination, the subjective evaluations. Thus 'Wa' was decidedly less certain in the 40–208 pair, but his record was poor only in the 208–40 combination. 'A' felt more certain in the order 104–144 than with 144–104, but he was more accurate in the latter combination. While the experimental differences were usually *less* than the subjective differences would have led us to expect, in the main the subjective evaluations were quite reliable.

6. How were the subjects able to estimate the mid-rates? Did they base the estimations on the '*immediate impressions*' received from the stimuli, or on *secondary factors?* Were their judgments of the immediate or the reflective type? If secondary factors had to be used to aid the judgments, what was their nature?

Six believed that they based their decisions in one combination or another on the *absolute impressions* received from the stimuli, on a 'sensation of difference,' or on the 'time sense' — one only when the differences were large, and one even when they were small — while five had to *reflect* or go through a process of comparison. Most subjects used a variety of *secondary criteria.*

The most frequently used were *movements* of the body or the bodily members (by all except three). The most frequent movement was by the finger, followed by the head, foot, body, tongue and throat. One kept time by articulating more or less for the two standards. One nodded the head for the series which had just elapsed in 104–144, and for every second click in 40–208, or moved the tongue upward or moved the two fingers for each beat. One swayed the body with the slow clicks, and one contracted the throat to retain the rate of the first standard. Three kept the first standard in mind by moving the finger, two doing this during the variable series. One moved the finger for the slow standard for the purpose of determining whether two of the variable beats corresponded with one finger movement, but found that the finger beats tended to assume the rate of the variable beats. He also described three lines of different lengths by means of finger movements in the air, but found this of no aid. Three "beat time" more or less for both standards. Closely related was the tendency to count the clicks, but counting proved to be of no aid. For one the rate of counting changed to the rate of the ongoing series.

Almost as many (seven) tried to *halve* or *double* the standards; *i. e.*, to select a rate which was half as fast as the faster standard and twice as fast as the slow. One proceeded to 'multiply the slow by two and divide the fast by two'; one 'grouped the faster into pairs and doubled the slower ones'; one tried to see if there were two beats in the variable for one movement of the tongue for each beat in the first standard, but the change to the second standard made him uncertain. The use of this factor was restricted largely to the 40–208 pair (mentioned five times, but only two times with 72–176 and once with 104–144).

Theoretically we should assume that this expedient was useless or misleading, for the process of doubling or halving does not give the correct mid-rate, as is seen from the following tabulation in which the faster rates in each pair are halved and the slower rates doubled:

Standards	40–208	72–176	104–144
Double the slower	80	144	208
Half the faster	104	88	72
	92	116	140

In practice, the mid-tempos actually selected were sometimes nearer the true mathematical mean, and sometimes nearer the halved or doubled rate, or the average of the halved and doubled rates. The results for the same subject were not always consistent. In all cases this factor increased the amount of the error. While we may thus say that it exercises an influence on the choice of the mid-tempo, particularly for the 40–208 pair, it does not do so to the same extent in all the part series: in one combination the subject may rely on this aid but not in another. This fact evidently accounts for the large difference between some of the part series found for some of these subjects.

The use of *visual imagery* was reported by three only. One imaged the pendulum weight at the top, middle and bottom of the pendulum for the three series, and also a line on the blackboard for the mid-rate; one had a vague perception of the distance between each beat; and one had a very vague impression of visual extent. On the other hand, five did not use any sort of a visual scheme. It is therefore probable that visual imagery did not exercise any determinate influence on the estimations.

As already seen five believed that they were, and five that they were not, able to *ideate* the mid-rate, to have in mind a *mental standard*—a sort of intuitive mental yardstick—by means of which to determine the mid-rate. The existence of such an 'ideal standard' can scarcely be postulated until this phase of the problem has been subjected to a more searching inquiry. There is material aplenty here for an independent investigation.

One thought that he *knew the rates* of the limiting stimuli in the 40–208 pair, and that he used this knowledge as a criterion, while two felt that their later estimations were influenced by earlier ones—such influence is discernible in some combinations but certainly not in others.

Two employed *objective criteria*, one conceiving of the mid-rate as twice as fast as the second's hand, and one having in mind a clock tick.

One *grouped* the first standard and variable, and then the variable and second standard and recalled the *contrasts*. He based his judgments on the 'contrasts.' One perceived the mid-tempo as a 'smooth rate,' while he was unable to adapt himself to the standards, particularly the very fast. One tried to keep both standards in mind, and to image a mid-rate 'which was neither very fast nor very slow.'

The *breathing* or *pulse rate* did not exercise any influence with any of the five subjects reporting on this point.

In view of the use of a variety of secondary criteria we seem justified in *concluding* that the process of equating appertains to the sphere of *reflective judgment* rather than to the sphere of *immediate sensation.* Probably fewer subjects than indicated by the introspections based their judgments purely on the immediate impressions of the stimuli (sensational factor). The criteria actually used varied more or less with the different subjects and the different standards, and also in their degree of effectiveness. The *movement* factor was perhaps the most effective, while the 'doubling' and 'halving' process led the judgment astray.

7. From other introspections the *following facts* appear. The tendency to *rhythmically group* the individual series of sounds was not very pronounced (reported by four only). The accuracy of the rhythmical combinations was not superior to the general averages, or to the subjects' non-rhythmical combinations, except in two cases.

The subjects were asked whether the series of beats appeared to them as '*durations*' (intervals) or '*frequencies.*' Two found with the fast beats that attention was on the 'number of beats,' or the 'rapidity,' or the 'frequency,' while with the slow beats one found that it was on the 'duration or length of the interval.' Four others seemed to regard the series as durations.

Attention seems to be directed nearly as often to the *sensation* as the *stimulus* sides of the experience. Seven attended more to the sensations in one combination or another (one explaining that he did not 'have the instrument in mind,' and one attending to the beats as they 'affected him') while five attended more to the stimuli in one combination or another (one 'visualized' the metronome).

At what point in the three series did the subjects form their opinion or arrive at their *decisions*? Nine usually or at times decided during the *course of the variable series* of beats, some in the early part and some in the latter part. Six did this only when the variable was not too near the mid-point. The decisions occurred more frequently in the variable series for the 104–144 (four mentions) and 72–176 (three

mentions) pairs than for the 40–208 pair (once). Of course, the decisions could be reached in the variable series only after the variable standard had become so familiar that it could be retained.

Ten arrived at their decisions usually or at times during the *course of the second standard*, more frequently in the early part than in the latter part of the series, and more frequently with the 40–208 pair (six mentions) than with the 72–176 and 104–144 pairs (four mentions each). Two had to wait for the second standard when the variable was near the mid-point; one had to wait because it was difficult to remember the 40 standard, which he usually ideated too fast; and one, after hearing the first combination in a pair, used the second standard only to verify his judgments. Two sometimes made their decisions *after the close* of the *second standard*—one when the variable was near the mid-point or when he was doubtful, and the other when his judgments would change during the latter part of the second standard.

While it is thus apparent that there is no one favorable moment in which the decisions are reached, the *nearer together the limiting stimuli are* (within our limits) *and the farther the variable is from the mid-point, the earlier is the judgment formed.* Most subjects decide in the variable series or in the early part of the second standard. Extremely few wait until all three series have been given.

Second Experiment

The Estimation of the Mid-Rate between two Tempos by the Method of Tapping (Method of Expression).

The instructions given were as follows: "I shall have the metronome click off a series of tempos, and then, after an interval of three or four seconds, a second series of tempos, which we shall refer to, respectively, as the first and second standards (or fast and slow, or slow and fast standards). When the metronome has been stopped and I have had time to start the kymograph, I want you, at a given signal, to tap off on this key, with a quick movement of the fore-

Second Experiment—Estimation of the Mid-Rate betwenn Two Tempos, by the Method of Tapping

Table VII

Standards: 40 and 208 (Beats per Minute)

S.	Time Order		Length of Interval Between Taps																Average, First to Fourth Sets			
			First Set				Second Set				Third Set				Fourth Set							
			Phases		Total Length	M.V.	Phases		Total Length	M.V.	Phases		Total Length	M.V.	Phases		Total Length	M.V.	Phases		Total Length	M.V.
			Down	Up			Down	Up			Down	Up			Down	Up			Down	Up		
J.	I.	*a*	.175	.247	.423	.050	.152	.232	.385	.007	.168	.268	.436	.073	.153	.291	.445	.035	.162	.267	.430	.041
H.	III.	*a*	.068	.826	.895	.031	.070	.868	.938	.026	.072	1.060	1.132	.028	.070	.842	.912	.039	.070	.899	.969	.031
D.	V.	*a*	.245	.368	.613	.022	.173	.291	.466	.013	.207	.317	.525	.015	.142	.467	.611	.024	.189	.361	.554	.018
Ave.			*.162*	*.478*	*.643*	*.034*	*.131*	*.463*	*.579*	*.015*	*.149*	*.548*	*.697*	*.038*	*.121*	*.533*	*.656*	*.033*	*.140*	*.509*	*.651*	*.030*
Wa.	IV.	*b*	.316	.203	.510	.015	.306	.230	.536	.017	.295	.283	.578	.026	.271	.272	.544	.028	.297	.247	.544	.021
S	II.	*b*	.036	.978	1.015	.046	.028	1.036	1.065	.036	.028	.962	.991	.018					.031	.992	1.023	.033
R.	IV.	*b*	.214	.264	.478	.019	.202	.265	.467	.013	.237	.260	.497	.012					.218	.263	.482	.015
B.	II.	*b*	.013	.366	.380	.011	.021	.381	.402	.107	.035	.303	.338	.010	.022	.365	.387	.027	.023	.354	.377	.039
Ma.	II.	*b*	.105	.530	.635	.012	.115	.672	.787	.031	.181	.457	.638	.023	.217	.638	.856	.037	.154	.574	.729	.026
Ave.			*.137*	*.468*	*.603*	*.020*	*.134*	*.516*	*.651*	*.041*	*.155*	*.453*	*.608*	*.018*	*.170*	*.425*	*.595*	*.030*	*.144*	*.486*	*.631*	*.027*
Ave.[1]			*.146*	*.472*	*.618*	*.026*	*.133*	*.497*	*.624*	*.031*	*.153*	*.488*	*.642*	*.025*	*.145*	*.479*	*.625*	*.031*	*.143*	*.494*	*.638*	*.028*
Standards: 208 and 40																						
Wa.	III.	*a*	.425	.266	.691	.014	.371	.230	.603	.015	.390	.221	.610	.029	.313	.235	.551	.016	.375	.238	.613	.018
S	I.	*a*	.037	1.031	1.068	.060	.039	1.142	1.181	.046	.040	1.145	1.185	.023	.031	1.030	1.061	.023	.037	1.085	1.122	.038
R.	III.	*a*	.166	.234	.401	.040	.204	.226	.431	.012	.156	.255	.412	.011	.204	.264	.468	.021	.182	.245	.428	.021
B.	I.	*a*	.016	.293	.310	.011	.016	.375	.392	.025	.013	.388	.402	.017	.018	.630	.648	.025	.016	.416	.432	.019
Ma.	I.	*a*	.138	.675	.814	.036	.090	.592	.682	.020	.070	.673	.743	.033	.218	.712	.931	.043	.129	.663	.791	.035
Ave.			*.156*	*.499*	*.656*	*.032*	*.144*	*.513*	*.657*	*.023*	*.134*	*.536*	*.670*	*.022*	*.157*	*.574*	*.731*	*.025*	*.148*	*.529*	*.677*	*.026*
J.	II.	*b*	.175	.460	.635	.037	.138	.457	.596	.023	.187	.550	.737	.035	.148	.687	.836	.033	.161	.540	.703	.032
H.	IV.	*b*	.032	.785	.820	.032	.045	.757	.805	.011	.037	.678	.716	.055	.067	.734	.801	.034	.045	.738	.785	.033
D.	VI.	*b*	.201	.325	.526	.013	.175	.275	.452	.021	.142	.271	.413	.009	.080	.283	.363	.011	.149	.289	.441	.013
Ave.			*.136*	*.523*	*.660*	*.027*	*.119*	*.496*	*.617*	*.018*	*.122*	*.499*	*.622*	*.033*	*.098*	*.568*	*.666*	*.026*	*.118*	*.522*	*.643*	*.026*
Ave.[1]			*.148*	*.508*	*.658*	*.030*	*.135*	*.506*	*.642*	*.021*	*.129*	*.522*	*.652*	*.026*	*.134*	*.571*	*.707*	*.026*	*.136*	*.526*	*.664*	*.026*
Ave.[2]			*.147*	*.490*	*.638*	*.028*	*.134*	*.502*	*.633*	*.026*	*.141*	*.506*	*.647*	*.025*	*.139*	*.532*	*.672*	*.028*	*.139*	*.510*	*.651*	*.027*

SECOND EXPERIMENT—ESTIMATION OF THE MID-RATE BETWEEN TWO TEMPOS, BY THE METHOD OF TAPPING

TABLE VIII

Standards: 72 and 176

S.	Time Order		Length of Intervals Between Taps: First Set Phases Down	First Set Phases Up	First Set Total Length	First Set M.V.	Second Set Phases Down	Second Set Phases Up	Second Set Total Length	Second Set M.V.	Third Set Phases Down	Third Set Phases Up	Third Set Total Length	Third Set M.V.	Fourth Set Phases Down	Fourth Set Phases Up	Fourth Set Total Length	Fourth Set M.V.	Average, First to Fourth Sets Phases Down	Phases Up	Total Length	M.V.
S.....	V.	*a*	.021	.711	.732	.022	.027	.603	.631	.091	.030	.576	.606	.010	.020	.515	.535	.015	.024	.601	.626	.034
H.....	V.	*a*	.053	.708	.762	.020	.073	.758	.833	.019	.156	.761	.917	.031	.236	.637	.873	.033	.129	.716	.845	.025
D.....	I.	*a*	.065	.251	.316	.010	.091	.245	.336	.082	.190	.295	.485	.013	.152	.275	.427	.011	.123	.267	.391	.029
Ma.....	III.	*a*	.112	.555	.666	.027	.096	.403	.498	.019	.105	.537	.642	.033	.075	.707	.781	.028	.097	.545	.642	.026
Ave.....			*.062*	*.531*	*.619*	*.019*	*.072*	*.502*	*.574*	*.052*	*.120*	*.542*	*.662*	*.022*	*.120*	*.533*	*.654*	*.022*	*.093*	*.532*	*.626*	*.028*
J.....	IV.	*b*	.196	.465	.661	.024	.244	.363	.607	.013	.303	.423	.726	.010	.203	.295	.498	.023	.238	.384	.621	.017
Wa.....	II.	*b*	.294	.253	.547	.031	.291	.213	.504	.030	.311	.221	.532	.019	.281	.231	.512	.024	.294	.229	.524	.026
R.....	II.	*b*	.185	.220	.401	.016	.202	.236	.440	.009	.184	.277	.461	.012	.183	.267	.451	.011	.188	.250	.438	.012
B	VI.	*b*	.023	.665	.690	.020	.014	.560	.574	.105	.015	.606	.621	.031	.022	.460	.482	.048	.018	.572	.590	.051
Ave.....			*.174*	*.400*	*.575*	*.23*	*.187*	*.343*	*.531*	*.039*	*.203*	*.382*	*.585*	*.018*	*.172*	*.313*	*.485*	*.026*	*.184*	*.359*	*.543*	*.026*
Ave.[1]....			*.118*	*.478*	*.596*	*.21*	*.129*	*.422*	*.552*	*.045*	*.161*	*.462*	*.623*	*.020*	*.146*	*.423*	*.569*	*.024*	*.138*	*.445*	*.584*	*.027*

Standards: 176 and 72

J.....	III.	*a*	.242	.548	.791	.026	.185	.510	.695	.021	.186	.716	.902	.028	.194	.443	.637	.035	.202	.530	.733	.027
Wa.....	I.	*a*	.250	.285	.540	.013	.280	.270	.550	.025	.307	.238	.541	.045	.272	.262	.540	.028	.277	.264	.541	.028
R.....	I.	*a*	.045	.375	.421	.013	.060	.358	.418	.021	.165	.281	.446	.015	.168	.262	.431	.016	.100	.317	.426	.016
B.....	V.	*a*	.023	.497	.521	.021	.021	.694	.715	.019	.013	.466	.480	.056	.016	.430	.446	.037	.018	.521	.539	.033
Ave.....			*.140*	*.426*	*.558*	*.018*	*.136*	*.458*	*.594*	*.021*	*.168*	*.425*	*.592*	*.036*	*.162*	*.349*	*.513*	*.029*	*.149*	*.408*	*.559*	*.026*
S	VI.	*b*	.026	.498	.524	.029	.027	.555	.583	.013	.023	.493	.516	.014	.020	.048	.509	.011	.024	.508	.532	.017
H.....	VI.	*b*	.086	.726	.813	.017	.135	.641	.776	.022	.227	.503	.730	.013	.108	.657	.767	.019	.139	.632	.771	.018
D.....	II.	*b*	.146	.237	.383	.012	.141	.336	.477	.022	.188	.228	.417	.024	.145	.230	.375	.007	.155	.258	.414	.016
Ma.....	IV.	*b*	.053	.765	.818	.023	.093	.627	.721	.033	.056	.661	.717	.020	.036	.632	.667	.031	.060	.670	.731	.027
Ave....			*.077*	*.556*	*.634*	*.020*	*.099*	*.539*	*.639*	*.022*	*.123*	*.471*	*.595*	*.017*	*.077*	*.399*	*.579*	*.017*	*.094*	*.517*	*.612*	*.019*
Ave.[1]....			*.109*	*.491*	*.596*	*.019*	*.117*	*.498*	*.616*	*.022*	*.145*	*.448*	*.593*	*.026*	*.119*	*.374*	*.546*	*.023*	*.121*	*.462*	*.585*	*.022*
Ave.[2]....			*.113*	*.485*	*.596*	*.020*	*.123*	*.460*	*.584*	*.033*	*.153*	*.455*	*.608*	*.023*	*.132*	*.398*	*.557*	*.023*	*.129*	*.453*	*.584*	*.024*

TABLE IX

Standards: 104 and 144

S.	Time Order		Length of Intervals Between Taps: First Set Phases Down	First Set Phases Up	First Set Total Length	First Set M.V.	Second Set Phases Down	Second Set Phases Up	Second Set Total Length	Second Set M.V.	Third Set Phases Down	Third Set Phases Up	Third Set Total Length	Third Set M.V.	Fourth Set Phases Down	Fourth Set Phases Up	Fourth Set Total Length	Fourth Set M.V.	Average, First to Fourth Sets Phases Down	Phases Up	Total Length	M.V.
Wa.....	V.	*a*	.294	.210	.504	.027	.298	.214	.513	.027	.298	.212	.511	.012	.295	.246	.542	.018	.294	.220	.514	.021
S.....	III.	*a*	.020	.621	.641	.028	.024	.613	.637	.012	.020	.555	.575	.015	.021	.588	.610	.017	.021	.594	.615	.018
R.....	V.	*a*	.226	.254	.481	.023	.232	.213	.445	.012	.222	.236	.458	.009	.233	.228	.462	.010	.228	.233	.462	.013
B.....	III.	*a*	.018	.494	.513	.033	.020	.453	.473	.034	.022	.570	.592	.025	.020	.592	.612	.023	.020	.523	.543	.029
Ave.....			*.139*	*.395*	*.537*	*.028*	*.143*	*.373*	*.517*	*.021*	*.140*	*.393*	*.534*	*.015*	*.142*	*.413*	*.556*	*.017*	*.140*	*.392*	*.533*	*.020*
J.....	VI.	*b*	.261	.337	.598	.020	.203	.321	.532	.020	.233	.264	.497	.019	.286	.312	.598	.021	.245	.303	.553	.020
H.....	II.	*b*	.047	.762	.810	.027	.036	.700	.736	.024	.042	.737	.780	.014	.030	.690	.720	.027	.039	.722	.762	.023
D.....	IV.	*b*	.088	.390	.481	.010	.111	.355	.466	.007	.180	.281	.462	.014	.162	.313	.476	.007	.136	.335	.471	.009
Ma.....	VI.	*b*	.065	.435	.500	.017	.061	.523	.583	.013	.056	.555	.611	.018	.053	.553	.607	.017	.059	.516	.575	.016
Ave.....			*.115*	*.481*	*.597*	*.018*	*.103*	*.479*	*.579*	*.016*	*.128*	*.459*	*.587*	*.016*	*.132*	*.467*	*.600*	*.018*	*.119*	*.469*	*.590*	*.017*
Ave.[1]....			*.127*	*.438*	*.567*	*.023*	*.123*	*.424*	*.548*	*.018*	*.134*	*.426*	*.560*	*.016*	*.137*	*.440*	*.578*	*.017*	*.129*	*.430*	*.562*	*.018*

Standards: 144 and 104

J.....	V.	*a*	.313	.437	.751	.026	.212	.366	.578	.041	.158	.365	.524	.035					.225	.388	.613	.034
H.....	I.	*a*	.032	.775	.807	.057	.042	.823	.866	.049	.036	.693	.730	.052	.025	.583	.613	.021	.033	.716	.752	.045
D.....	III.	*a*	.175	.232	.407	.012	.191	.265	.456	.013	.172	.243	.415	.011	.047	.347	.394	.011	.141	.268	.414	.012
Ma.....	V.	*a*	.052	.498	.551	.014	.034	.511	.545	.007	.042	.467	.510	.017	.061	.468	.530	.020	.047	.485	.533	.014
Ave.....			*.143*	*.485*	*.629*	*.027*	*.119*	*.491*	*.611*	*.027*	*.102*	*.442*	*.545*	*.029*	*.044*	*.466*	*.512*	*.017*	*.111*	*.462*	*.578*	*.026*
Wa.....	VI.	*b*	.298	.222	.521	.023	.294	.248	.541	.016	.264	.256	.520	.015	.274	.259	.533	.012	.282	.246	.528	.014
S.....	IV.	*b*	.020	.500	.520	.020	.021	.478	.500	.020	.023	.446	.470	.015	.022	.423	.450	.022	.021	.461	.483	.019
R.....	VI.	*b*	.236	.241	.487	.014	.226	.247	.475	.014	.243	.245	.488	.012	.224	.248	.472	.008	.232	.245	.478	.012
B.....	IV.	*b*	.023	.725	.748	.039	.020	.542	.562	.034	.025	.456	.476	.040	.020	.554	.574	.030	.022	.568	.590	.035
Ave.....			*.144*	*.422*	*.569*	*.024*	*.140*	*.379*	*.519*	*.022*	*.139*	*.349*	*.488*	*.020*	*.135*	*.371*	*.507*	*.018*	*.139*	*.380*	*.519*	*.020*
Ave.[1]....			*.143*	*.453*	*.599*	*.025*	*.129*	*.435*	*.565*	*.024*	*.120*	*.395*	*.516*	*.024*	*.096*	*.411*	*.509*	*.017*	*.125*	*.421*	*.549*	*.030*
Ave.[2]....			*.135*	*.445*	*.583*	*.024*	*.126*	*.429*	*.556*	*.021*	*.127*	*.410*	*.538*	*.020*	*.118*	*.427*	*.546*	*.017*	*.127*	*.425*	*.555*	*.020*

Explanations follow Table IX.

Explanation of Tables.—Unit of measurement: 1 second. S.: subject (males). Time order: temporal succession in which the different combinations were given. I. = first. II. = second, etc. *a* and *b* indicate which of the standards in a given pair, the fast or the slow, was given first; *a* = first; *b* = second. Phases: refers to the *down* and *up* movement of the finger in tapping the key. Total length: the total length of the tapped mid-interval, the distance from the beginning of one tap to the beginning of the next, thus including both the down and the up phases. First set: this gives the average results for the first series of eight or nine taps. Second set: ditto for the second series of eight or nine taps, etc. The one set followed the other in close succession. M.V.: mean variation, computed between the individual taps for each subject in each set. Ave.^{1}: the average for the fast-slow or slow-fast combination of a given pair. Ave.^{2}: general average for a given pair, average of the two Ave.^{1}'s. Pair: the two limiting stimuli which were given together; *e. g.*, 40 and 208. Combination: the order in which the standards (limiting stimuli) were given; *e. g.*, '40 and 208' followed by '208 and 40'; or '208 and 40' followed by '40 and 208.' The standards were given in the order indicated. Thus '40 and 208' indicates that the 40 rate preceded the 208 rate.

finger of the right hand, a tempo which you estimate to lie midway between the rates of the standards—that is, the mid-tempo." The kymograph drum was allowed to attain its speed before the subjects were asked to tap—an interval of five seconds, more or less, after the second standard had been given.[1]

The electrical tapping reaction key was so constructed that the electrical circuit was closed the moment the key was depressed, and broken again the instant the key returned (by spring action) to its original position. In Tables VII. to IX. the time the key was depressed ('down' phase of the finger movement) and the time it was at rest ('up' movement of the finger), are given separately. The length of the estimated mid-interval is, of course, the sum of the make and the break records on the drum—that is, the entire time from the beginning of one tap to the beginning of the next.

The reactions were recorded by means of a Deprez marker on the smoked paper on the drum of a Ludwig-Baltzar kymograph. Time lines, made by a 100 VD fork, paralleled all the reaction records. The measurements are accordingly in terms of seconds. It is thus necessary to express the mid-

[1] Three regarded this interval as about right (S, H, R), and two sometimes felt that it was rather brief (D, Wa).

rates in the tables as *interval lengths* or *durations* in fractions of a second, instead of as *frequencies* in terms of the number of beats per minute, as was the case in the first experiment.

After the subject had tapped eight or nine times the kymograph was stopped, and the subject was required to listen to the same standards again, given in the same order, after which he was asked to tap off his estimated mid-rate just as before. This procedure was gone through two more times (in almost all cases) so that the subjects listened to the same pairs of standards in the same order, and tapped the estimated mid-tempo, in four successive sets. The successive sets of eight or nine taps are averaged in the tables in the columns headed 'First Set,' 'Second Set,' etc. It is therefore apparent that altogether eight successive sets of measurements were made for each pair of standards: 4 sets in the fast-slow combination (*e. g.*, 208–40), and 4 sets in the slow-fast (40–208)—or 24 sets for all three pairs.

Results.— (1) The produced mid-intervals, based on the averages for the four sets[1] (last four columns), are invariably *longer (slower) than the arithmetical mean.* The error, in terms of interval length, amounts to .168 sec. for the 40–208 pair, .101 sec. for the 72–176 pair, and .071 for the 104–144 pair. In all averages the mid-rates are *nearer the geometrical than the arithmetical mean.* The produced mid-interval for the 40–208 pair is exactly the same as the geometrical mean, while it is .054 sec. slower for the 72–176, and .065 sec. for the 104–144 pair.

Three facts here merit attention: first, the fact that the errors in the two experiments are in the *same direction*—the estimated rates being *too slow;* second, the fact that the estimates are *nearer the geometrical than the arithmetical mean,* particularly with the method of production; and third, the fact that the *errors are noticeably larger in the second experiment*—larger by .091 sec. in 40–208, by .084 in 72–176, and by .064 in 104–144. (The estimations in the first experi-

[1] Limitations of space render it necessary to restrict the discussion to the general averages for the four sets (last four columns). More detailed data are available for the reader in the tables.

ment were, in terms of interval length, for the pairs in the order enumerated above, .56, .50, and .49 sec.)

The experimental inaccuracy is paralleled by *introspective uncertainty* in this experiment. Five regarded their productions as guesses, one having no definite idea of the mid-rate, two only being certain that they were somewhere between, and another that his taps were different from the last standard. Only two felt fairly certain. Thus both experimentally and introspectively the method of *tapping* (producing) the mid-rate is less accurate than the method of selecting it from a series of variable clicks. This is probably due to the aid received from the step procedure in the first experiment, for of the five subjects who served in both experiments three regarded the second as easier, and none found the second experiment fatiguing while a considerable number experienced fatigue in the first.

(2) As seen in the above figures, the *absolute inaccuracy increases as the distance between the limiting stimuli increases*, the error for 40–208 being one and two thirds times as large as for 72–176, and two and one third times as large as for 104–144. But relatively to the length of the interval between the limiting stimuli (*relative* inaccuracy) the order is the exact reverse, the errors amounting to 13 per cent., 20 per cent. and 43 per cent. for the pairs in the order given above.

The same difference appears in the mean variations. The absolute M.V's increase as the distance between the stimuli increases (.020, .024 and .027 sec.), while the relative M.V's decrease (.12, .049 and .022).

Two regarded the 40–208 pair as the *easiest* (one because he felt certain with this pair that he was not very near either standard though he did not know whether he produced the correct mid-rate), and two the 72–176 pair. One regarded the 40–208 pair as the *hardest*, and three the 104–144 pair (one because the standards were too close, so that he tended to give one of the standards, one because he was not absolutely sure that he was between, and one because it was hard to retain the fast rate). One expressed no preference, while one regarded all as 'unsatisfactory.'

We therefore conclude that *the absolute inaccuracy of estimating the mid-point increases as the distance between the standards increases*—a uniform result in both experiments. But there is more or less disharmony between the two methods in respect to the *relative inaccuracy* and the *introspections*. Peculiarly in both experiments the pair which gave the least absolute inaccuracy, 104–144, was adjudged the most 'difficult.'

(3) Just as in the first experiment, giving the *slow standard first produced slightly more accurate estimations* than giving the fast standard first, for 40–208 (a difference of .026 sec.)

and 72–176 (a difference of .001), but not for 104–144 (a difference of .013 sec.).

But the M.V's were slightly smaller when the fast standards were given first, except for 104–144; and introspectively the fast was *preferred* first by three, one when the 'faster rate was very fast because it was easier to get a rhythm of this and put it into the slow,' and two because it was easier to base the judgment on the slow. Three had no preference, and one was uncertain.

Five said that they *attended* more to, or were more influenced by, the *last* standard, one 'really making his tapping different from this,' and one feeling certain that his rate of tapping was different from the last standard at least. Two were more influenced by the *slow* standard and one by the *fast*.

Altogether one may conclude from both experiments that *the order in which the slow or fast standards is presented is not very material.* There is a slight difference, but the direction of the central tendency is not very clear. It seems to be largely a matter of personal idiosyncrasy.

(4) The *combination that was given second* for any pair was *more accurate* than that given first.

To illustrate what is implied in this statement: the combination 40–208 was given first to J, H and D (general average .651 sec.), and second (that is, after 208–40) to Wa, S, R, B and Ma (average .631 sec.); while 208–40 was given first to Wa, S, R, B and Ma (average .677 sec.), and second to J, H and D (average .643). The tapped interval lengths for all six combinations were as follows:

	Interval Length	M.V.
First	.617	.026
Second	.589	.022
Difference	.028	.004

It is seen that both the inaccuracies and the M.V's are slightly smaller in the combinations given second in order. There are only two exceptions to the rule among the different combinations (104–144 and 72–176), so far as the average values are concerned, and none so far as the M.V's are concerned (though two are equal). This gain is probably a *practice* effect. When the subject began with the second combination of any pair he already had made about 35 estimations with the first.

(5) The *degree of accuracy* with which different subjects are able to determine the mid-rate *differs considerably*, in this method as in the first. The largest error in the 40–208 pair amounts to .639 sec., which is .156 sec. more than the length of the mid-interval; in the 72–176 pair to .362; and in the 104–144 pair, to .279 sec. On the other hand, six of the sixteen records in the next to the last column (average for all sets) for the 40–208 pair, six for the 72–176 pair, and nine for the 104–144 pair, are correct within approximately .05 sec.

Contrariwise, the *subjects' evaluation* of their own estimations were not very reliable with the second method. Two were fairly certain of their judgments. But the errors in six of the final averages for one of these are large, while the absolute errors in the pair which he considered easiest (40–208) are larger than in the pair which he considered hardest (104–144). The errors for the other subject are all large (S). For the five who were uncertain, fully half of the estimations are fairly accurate for three, while for the other two some of the estimations are approximately accurate.

We may thus conclude that the *individual variations* are greater and the *subjective evaluations* less reliable in the method of expression (tapping) than in the method of impression (selecting mid-tempos from variables). It is well to emphasize the fact, however, that the tapping was very regular for most of the subjects. In the second experiment the M.V's could be computed between the individual reproductions in each set for each subject, and for only one do they reach .05 sec. (B, 72–176).

(6) The pressure or *down movement* of the finger in tapping (make phase of records) was considerably *shorter* (excepting for one subject) than the up movement. With the 40–208 it lasted only 27 per cent., with the 72–176 pair 28 per cent., and with the 104–144 pair 30 per cent. as long as the up movement.

(7) The following were the *average values* for the *produced mid-rates* for the *three* standards in each of the *four successive sets* (each set, as stated, consisting of eight or nine determinations):

	First	Second	Third	Fourth
Mid-interval....	.606	.591	.598	.592 sec.

The estimations in the second set are more accurate than those in the first set, those in the third set less accurate than those in the second, and those in the fourth more accurate than those in the third and about the same as those in the second set. In the 104–144 pair all the three later sets are more accurate than the first, but only two each in the 72–176 pair and in the 40–208 pair.

Likewise in respect to the *average regularity* for the three pairs: the M.V. is somewhat less in the third and fourth sets (.027 sec.) than in the first (.024), but greater in the second (.027). In the 104–144 pair, again, the regularity is better in all the three last tests than in the first, but in the 40–208 pair the regularity is better in only two of the later sets, while in 72–176 there is a consistent loss.

It is thus apparent that there was a *slight improvement in the estimations and a slight gain in the regularity in the later estimations—a slight practice effect*—but neither is regular or consistent except in the 104–144 pair.

(8) The *methods* used to estimate the mid-point were as follows:

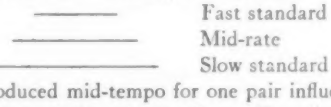
Four believed that they based their estimations on the *immediate impressions* received from the stimuli (one only when the standards were far apart, and one without any reliable or absolute feeling of the mid-rate). On the other hand, three did not think that their judgments were based on the absolute impressions, while two stated that the judgments were of the reflective type—a matter of mental calculation, or of counting.

Practically all the subjects made use of *kinæsthetic factors*—movements of the body, or tongue, or head, or finger. Five used the movement criteria with *both standards:* one moved the head, finger or body (for the slow rates) in time with the standards; one nodded the head or moved the tongue to the first standard and moved the finger to the second standard; three tapped with the finger for both rates, one estimating the mid-tempo from the finger beats. Two felt that the movements or muscular efforts were an aid in that they reinforced the memory, but one felt that they did not help as he tended to beat too fast (his estimations, however, were too fast only with the 40–208 pair). One, on the other hand, moved his finger during the *first standard* only, while one moved his hand to his '*ideal mid-rate*,' but found the value of this to be indifferent. One, who made no conscious movements, counted and found this helpful. One tried to 'measure the duration of the feeling of expectancy' with the 40-rate, and used this as an aid.

Four employed some scheme of *grouping or halving*. One counted the fast clicks in fours and continued to count during the slow standard, to see whether the counts would fall on every second or third click; he sometimes 'grouped the fast into the slow beats and halved the result for the mid-tempo,' and he sometimes tried to 'feel the distance between the third and fourth clicks.' Another tried to determine 'how many fast clicks went into a slow interval and took one half of the result,' but this did not prove satisfactory except for 40–208. One counted in eights, probably only as an involuntary tendency to rhythmize, and one 'grouped the 208 rates in halves' but found this a hindrance.

Two believed that they were, and six that they were not, guided more or less by an '*ideal standard*' for the mid-rate (one got in his 'auditory imagination' an image of the 'rate between'). One 'merely had an impression of what it ought to be.'

Only three were conscious of using any *visual imagery:* one visualized the tapping of a hammer; one visualized the mid-rate as a line dividing off a spherical area; and one had a vague image of a truncated triangle effect, thus:



Two felt that the produced mid-tempo for one pair influenced the judgment for the other pairs. One considered this a habit effect. The alleged influence is perhaps apparent in 'R's' averages, but only slightly, if at all, in 'Ma's.' 'Ma,' however, had a tendency to change his tapping rate, because he felt that he was beating too fast, but his M.V's are larger than the average only in some cases.

Comparison of the two methods (first and second experiments) justifies the following *conclusions* respecting the *process* of estimating the mid-tempo:

First, all subjects rely on some form of *secondary criteria.* Some try a considerable variety of aids, others only a few. By a process of selection and elimination, aids found useless are abandoned and those believed to be valuable are retained. The reactions are of the 'excess discharge' type.

Second, accordingly—although about half the subjects in each experiment report that the estimations were based on the immediate impressions received from the stimuli, and although about half in the first experiment and one third in the second report that they were guided more or less by an ideal standard of the mid-rate—the *judgments are mainly, if not entirely, of the reflective type.*

Third, the *stable criteria* in both methods may be reduced to two, namely kinæsthetic or motor factors, and a process of halving or doubling the standards (grouping). In the first method the movements seem to be made more frequently in time with the first standard, while in the second method the movements are usually made for both standards.

Fourth, *visual imagery* plays a very subordinate rôle in estimating the mid-point between tempos.

Fifth, the *value* of the different criteria employed varies, as shown by the experimental results and the introspections, with different subjects and with the distances between the pairs of standards.

(9) The *difficulty* experienced most frequently was the *inability to retain the standards.* Two found it particularly hard to retain the *first standard* (the 'second tended to drive out the first'); and one to retain the *fast* when the standards were close together.

Three found it difficult to get any impression of the *mid-rate,* and four to avoid the tendency to *tap too fast.* As a matter of fact, five of 'R's' and 'D's' and two of 'B's' six estimations (general averages) were too fast, while all of 'Ma's' and four of 'B's' were too slow. One found it difficult to keep from *basing his judgments* on the last standard only (three judged more from the last standard than the first); and one, to avoid adopting one of the standards.

Both methods thus make it evident that the *chief diffi-*

culties in estimating mid-tempos are to retain the standards, particularly the first and the fast, and to ideate, conceive, or know where to locate the mid-tempo.

(10) Only three seemed clearly to feel any *tendency to rhythmize* the clicks. Of these, all the averages of one, but only one each of those of the other two are superior to the average estimations of all subjects. Four reported that they perceived no tendency toward rhythmical grouping. Both experiments thus show that *the tendency to rhythmize a uniform series of clicks in an experiment of this sort is weak, and that, when present, it usually does not increase the accuracy of the estimations.*

(11) Six subjects apprehended the succession of clicks as *frequencies.* (One only the rapid clicks, and one the 40–208 pair.) 'Ma's' tendency to count indicated to him that he apprehended the series as frequencies. On the other hand, three apprehended the rates as *durations* or interval lengths, one only the slow standards, and one the two pairs of standards closest together.

The two methods agree in indicating that there is a tendency to regard the *fast series as frequencies* and the *slow as durations,* but that the subjects often are *uncertain* as to whether they apprehended the series in the one way or the other. It is observable that in the first method more regarded the beats as durations while the reverse was the case in the second method.

Whether the one type of apprehension makes for *greater accuracy* of estimation than the other is difficult to determine, because many subjects will regard one standard of a given pair as durations and the other as frequencies. If we consider only those who apprehended the successions in one way, and sum up the number of individual averages (separately for those who apprehended the successions as durations and for those who regarded them as frequencies) which are superior and inferior to the average estimations for all subjects, it is found that in the first method those who regard the beats as *frequencies* have 11 superior to 12 inferior counts; and those who regarded them as *durations* have 20 superior and 20 inferior counts. The corresponding figures in the second test are, for the frequency group, 15 superior to 11 inferior, and in the duration group 10 superior to no inferior counts. These differences are not material. Some subjects in each group excel in all their averages, and others are always poor, while some subjects in each group are better than the average in some pairs but not in others.

It is thus apparent that so far as concerns the accuracy

of estimation it is *immaterial whether the clicks are apprehended as frequencies or durations.*

(12) Two subjects believed that they *attended* more to the stimuli than the sensations while five did the reverse. One centered the attention mostly on his counting.

Both experiments show that the *subjects are not always certain* as to whether their attention is mainly objective (by which I refer to attention mainly directed to the stimuli) or subjective (directed to sensation), that the same subject may pronounce differently at different times or for different pairs, but that, in the main, *attention is centered nore on the sensation side than on the stimulus side.*

Here again different subjects center the attentiion differently at different times so that it is difficult to determine whether the subjective or objective type is the *more accurate.* If we disregard the four in the first experiment who are now objective and now subjective, and sum up the number of superior and inferior averages in each group, as was done above, it is found that in the first experiment the *subjective type* has 13 superior to 21 inferior counts, and the *objective type* 6 superior to 4 inferior. In the second experiment the subjective type has 16 superior to 14 inferior marks, and the objective type 10 superior to 2 inferior. The results are thus discrepant in the two experiments. Some indeed in each type are uniformly accurate and others are uniformly inaccurate.

We conclude that the *accuracy of estimating mid-tempos does not depend on whether the subject attends chiefly to the sensation or the stimulus side of the experience.*

(13) Three arrived at their '*decisions*' *after the close of the last standard*, while two decided *toward the close of the second standard.* Three scarcely regarded the matter in the light of a *decision*: one simply struck off a rate and then judged of its satisfactoriness; one found it impossible to form a decision and simply waited for the signal to tap; one felt that to have formed a decision he would have had to take time to make a comparison.

The instrumental procedure in the two experiments was so different that a comparison on this point would be meaningless. But it appears that *in the first method the subject goes through more of a process of 'forming an opinion or reaching a decision,' than in the second method.*

LITERARY SELF-PROJECTION

BY JUNE E. DOWNEY

I

Consideration of the imaginal forms that the self may assume would appear essential to a complete understanding of the psychology of the self. Yet this specific form of imagery has been unduly slighted.

A popular article by the present writer has described[1] the conceptual images of the self as reported by a number of observers. The most common representation appeared to be visual, and of such visualization the most frequent forms were reproductions of the reflection in the mirror or of a favorite photograph, the latter of which might be absurdly out of date, unduly flattering, or the reverse. The shadow also was found to be a frequent symbol of the self. In the visual images of the self peculiar abbreviations of the person might occur, so that the visualized self appeared decapitated or in schematic form, reduced perhaps to a pair of feet or one shoulder, or described as a dark square topped by a pompadour. Curious substitutions occurred, as in the case of the sister whose visual representation of herself took the appearance of her brother. The concept of the self was often described as an attitude; the attitude, for instance, of meditation—a shadowy chin felt to be resting against clasped hands.

Now the report of such symbols must have value as a revelation of temperament. This is evident in many instances as in that of the diffident woman whose self-image is always clad in an unbecoming coat, or in that of the young girl who always appears in her mental rehearsals gowned in her favorite party dress. A young man, with an ambition to be a physician, includes in his visual representation of himself a doctor's case. A college girl whose concept of

[1] 'The Image of the Self.' *The Educational Bi-Monthly*, Vol. IV., No. 1 (1909).

self is described as the feeling of a semi-masculine stride finds that this idea of herself dominates her in her selection of clothes even to the detriment of her judgment as to what is becoming to her objective self.

Some interesting facts relative to the representation of the self in a particular situation have been reported by Professor Martin[2] in her investigation of the images serving as cues to voluntary movement. Thus the transfer of the visualized movement to another person or to an animal was found to occur not infrequently. The writer's own experience as subject in a similar test seems instructive. A visual report from the body is found to be present always and the visual cue for a movement and the movement itself become inhibited if the part of the body that is to be moved is in a contradictory position. Frequently a double visualization appears. Thus, the command, "Raise both arms," brings a clear visual report of the two arms stationary with another shadowy pair in motion. The bearing of such facts upon the individual volitional type must be important.

II

The present report concerns itself with the personal reference involved in the appreciation of literary material and, in general, bears upon the question of the relation of the self to æsthetic experience, a question that, theoretically, has been productive of much discussion.

The situation utilized in gathering the reports was an extensive report upon the imagery aroused by the silent reading of fragments of poetry or by hearing such fragments read aloud. Two groups of subjects were used. There were eight subjects in the first group, all of whom had some training in experimental introspection. The second group contained six subjects of more extensive psychological training than those of the first group.

The members of the first group (*A–H*) reported on one hundred and ten fragments of poetry read silently, and

[1] Martin, L. J., 'Zur Lehre von den Bewegungsvorstellungen.' *Ztschft. für Psychol.*, Bd. 56 (1910).

on forty more fragments read aloud to them. Among the instructions given them was one taken from Professor Martin's investigation "Uber ästhetische Synästhesie."[1] "Do you experience a posture or movement of an object described as if it were your own posture or movement? If so, in what part or parts of the body is the posture or movement felt?"

In addition to such report the subject was asked, several weeks later, to arrange the hundred and ten fragments in three groups relatively to the vividness with which the subject projected himself into each fragment, with a fourth group for the fragments in which no self-projection occurred. An introspective report on the kind of self-projection was also asked for, self-projection being defined as any form of explicit reference to self, relative to the imaginal content. From this grouping the experimenter was able to determine the frequency and vividness with which self-projection was experienced.

The second group (*I–N*) reported the imagery aroused by the silent reading of one hundred fragments, the personal reference being inferred indirectly from detailed questions relative to the imagery aroused. In addition, thirty-one fragments were read silently with instruction that self-projection be reported whenever it occurred. The following forms of personal reference were noted as possible: Definite orientation relative to the imaged scene; felt posture; felt movement; organic identification with character or object described; cutaneous reference; visual and auditory self-reference.

It is evident that the situation made it possible to note cases of æsthetic Einfühlung or empathy with special reference to an imaginal content aroused by poetic material. No questions, however, referred specifically to such empathy and only one subject (*M*) of the fourteen gave answers whose phrasing showed familiarity with the doctrines of Einfühlung.

The self-projection on which a report was sought was, however, more inclusive than empathic experience. Visual

[1] *Zeitschrift für Psychologie*, Bd. 53 (1909).

self-projection in which, for instance, the subject sees himself as part of a visualized scene or as spectator of another's actions is an impressive form of self-projection but very different from an empathic projection. Again, although such self-projection is more inclusive than empathic projection, it is less inclusive than self-consciousness.

A general survey of the reports from the fourteen subjects shows, first of all, a noteworthy difference in the number of times explicit self-reference is reported. *C*'s number of self-projections is only 19.3 per hundred; *E*'s is 62 per hundred.

It is also interesting to note that such self-projection occurs with least frequency, in the reports both of the first and of the second group, in just that subject (*C* of the first, *K* of the second group) who gives the greatest number of memory images. The memory images of *C* bear chiefly the place-index, those of *K*, the time-index; for neither is the personal reference acute. The memory-consciousness appears often to be as objective in its reference as is a perceptual consciousness.

The first form of self-projection to be considered in detail is that of visual self-projection, or the seeing oneself in the midst of a visualized scene as actor or spectator. Rusk[1] in his tests on mental association in children found visual self-projection in the boy's imagery with what seemed to him surprising frequency. Several adults who were subjects in the present test reported visual self-projection as frequently as did Rusk's boy-subjects. In the tests where questions were asked specifically as to self-projection, *J* projected himself visually twenty-six times out of a possible thirty-one.[2]

Some details relative to this form of self-projection are of interest. Usually the self is seen as a more or less vague figure of the proper sex with little that is specific in the way of facial or other detail. For *B*, the self was merely a vapory figure seen in a given posture. *F*'s visualization of the self

[1] Rusk, R. R., "Experiments on Mental Association in Children." *The British Journal of Psychology*, Vol. III., Part 4 (1910).

[2] A curious example of what appears to have been visual self-projection of hallucinatory vividness is to be found in the biographic accounts of Shelley's apparitions of himself.

was slightly more specific and always clad in a reproduction of the garments she was wearing at the time of report. *E*'s visual self-projection was very specific. This self was definitely placed and seen from a definite position. Sometimes, this self would appear close at hand and life-sized. On such occasions the details would be vivid and a complete description could be given of the appearance of the self, such as style of hat worn, color of dress and the like. At other times, the self appeared far off, from half a block to half a mile. It would then seem indistinct and reduced in size. The following reports show the nature of *E*'s self-projection.

Visual, dark picture; by the sea; wind blowing. Optical movement of sea. Visual and felt self lying by the sea. Felt the wind on the face. Self seen from afar off; dim figure.

Again,

Visualized and felt self on the shore; optical movement of waves. Saw and felt the sand falling through fingers; sad relaxed feeling. Saw self distinctly from behind; self wore white dress and a big floppy straw hat.

H is subject to the distance-illusion. At times her surroundings retreat to a considerable distance, becoming smaller as they retreat. This experience of *H* is of interest in this connection because it is evident from her introspective reports that visual self-projection is of frequent occurrence and with this peculiarity, the visual image of self is often very tiny although no distance effect occurs. In general, *H*'s visual images are small and placed about two feet from her. A report of hers follows.

Visual image of self sitting. Dark room. Saw lap, book, and side of chair. Seen to right. Figures reduced in size. The visualized self very small, about eight inches tall, but localized in my actual position, that is, at my side, on chair on which I am sitting.

H has a circular form for the months of the year. The circle has a radius of about four feet with *H* localized at the center. If *H* hears a month of the year mentioned, she immediately localizes herself and the visualized scene at the proper place on the circle. Things appearing in the circle are diminutive because so many of them must be crowded into small compass. One of the poetical fragments made mention of the month of June. The report on it reads:

The month of June is to left, in corner of room; its position in the month-form. Self is first projected kinæsthetically to the corner. Then the visual image of self appears, very small, about two inches high, facing east.

J, as mentioned above, gave a surprising number of visual self-projections. *J*'s orientation with reference to his visualized self is very definite; so too is his orientation as observer of this visualized self. The distance of the visualized self from the observing self may vary from a few feet to one or several hundred yards, in which case the figure appears properly reduced in size. Such a distinct orientation both of the observing self and of the visualized self that is observed suggests the possibility of a double visual self-projection and such *J* reports as a not infrequent experience. Thus, in anticipating a trip, *J* would see himself going to meet himself. Self number one would be kinæsthetic with a visual glimpse of the feet and self number two a schematic figure approaching self number one from the opposite direction.

J's reports bring up the question of the relation of the visual self to the kinæsthetic or felt self and to other forms of self-reference as, for instance, cutaneous imagery. *J* may feel the posture, movement and tactile sensations or images instead of projecting them into the visualized self. At times, there is an oscillation of visual and kinæsthetic self which fail to fuse as in the following report.

See self from behind standing on beach, facing west. Has just thrown shell into water. Visual and kinæsthetic consciousness of right arm flexed; tension in back felt. No fusion of visual and kinæsthetic consciousness. Sometimes an actual oscillation. Self seen about one hundred yards off. Size reduced one fourth.

Again, *J* may get a double cutaneous report from his position as the observer and as observed.

See self lying on the ground on back. Feel the ground against back. As observer, standing. Get cutaneous sensations from both bottom of feet and back against ground. Cutaneous imagery for both visualized self and observing self.

Another report reads:

Kinæsthetic image of walking behind visualized self which appears as a small barefoot boy walking west. Cutaneous images from bare feet.

The double reference appears again in the following:

I am standing by a chair in which my visualized self is sitting. As observer I feel the movement of tying the handkerchief. As observed I feel its pressure on my forehead.

Visual self-projection was frequently reported by *I* also. Often, however, only a part of the body appeared or there were peculiar complications with kinæsthetic imagery.

To illustrate the first statement:

Image of two hands with pencil, pointing out the color of shell.

Again,

Hint of visual self, headless, with legs but no feet, trunk mainly. Shiver felt but no shiver to visual self.

To illustrate the second statement:

Feel asleep in hammock. Feel self and see hammock but can't see self in hammock.

This failure to visualize self, *I* reports as a curious experience.

At times, *I* reports a fusion of kinæsthetic and visual self; at times, a separation.

The following report illustrates fusion.

Feel of movement projected into the visual image of the self. Standing on left leg; right arm drawn back. Jerk of throwing shell felt.

The next report shows a different condition.

Visual of self sprawled out. Feel of posture belongs to actual body.

Other subjects also reported interesting relations between the kinæsthetic and the visualized self. The following reports were given by *F*.

Feeling of self sitting and dreaming. No other imagery. Visual self to the left of actual self and dressed as I am at present. Posture projected into this visualized self.

Again,

Visual image of self walking through forest. Visual scenery of last summer. Self that is seen is not the self felt walking, the second self is felt in space and back of the other self about one block distant.

C reported that the kinæsthetic self was felt to be in the margin of the picture; the visualized self at the focus. For *C*, visual and kinæsthetic self never coalesced; a fluctuation from one to the other would occur. The visualized self was not seen in any great detail and at times led a precarious existence as shown by the following example.

Saw self standing on the shore; mountain cataract; then scene shifted, self still there but changed by close of fragment to another person. At first, a distinct picture of self; at close, just the form of a man.

Self-reference in the form of definite orientation relative

to an imagined scene occurred with great frequency. Sometimes, such orientation is merely a vague feeling of position relative to the objects described; sometimes, further kinæsthetic elements are added. One walks the length, for instance, of a banquet hall and feels beneath the feet the 'soft wool-woofed carpets.'

B reports:

> Entered room, orientation confusing. Was whirled from center to left of room. Finally, took position in the left corner of a square room. Censers were on either side of room and there was auditory crackling. At last, saw mirrors appearing one at a time in wall, each reflecting a censer. This image caused a dizzying shift to a round room because the mirrors were better suited to a round room. The rapid shift in orientation peculiar. Beautiful room but confusing kinæsthesis.

Tactual self-reference was frequently experienced by other observers than *J*. For several subjects, the mention of wind or rain always induced a cutaneous consciousness of self. Such tactual self-consciousness was particularly noticeable in the case of *A* and *L*. *A* frequently reported that the wind was felt blowing through the hair or brushing the cheek. *A*'s self-projection in the scene described often took the form of tactile images of grasping something with the right hand. This tactile reference accompanies visual imagery of much richness.

> I saw a field of grass and flowers with cloud shadows racing over them; then the sea under the clouds. I could see rain and dew lying silver on the grass and leaves, but there was no auditory imagery whatever. There was one tactual image of plunging my hand into the dewy grass.

Tactual empathy was reported by *A* and *L*. Thus *A* writes of the imagery induced by reading the description of a well:

> A picture of the well, with big stones around it, and the sunshine dancing in the water and lighting up the walls as if someone had dropped something into it. Tactual images of plunging my hand into the water, and of drinking it.

A description of a fountain always induced a feeling of coldness for *L*, as did the description of snow-clad mountains.

Akin to the tactual self-reference was the acute bodily consciousness brought on by organic suggestion. A heightened consciousness of cardiac, respiratory, and other organic sensations was frequently reported by *L* and *D*. An emotional identification of the self with the emotion portrayed was also frequently experienced by these subjects.

Auditory self-reference was rare. *K* once hears herself laughing, and *J* hears himself whistling once and once hears himself grunting. Probably the best example of auditory self-projection is to be found in those cases where the reader hears each fragment read aloud in his own voice. Thus, *D* hears her own voice in varying cadence. There are emotional fragments in which consciousness is preoccupied wholly with inner elocution. The words read are expressive of her own emotion; she hears and feels herself saying them with dramatic nuances.

The social implication in these reports of self-projection is often evident as shown by the following examples from the reports of *E* and *F*.

> Experience myself as dressed in beautiful white things and walking in a shady park. Meet several people who look staring at me but do not speak.
>
> Visual image of a rainbow across the sea. People around self. Visualized self on shore, standing and looking out. People known to be talking. Visualized self feels as though among *many people*.

A report of particular interest in that the fragment, which was descriptive of a rainbow, gave no social suggestion.

III

How the kinæsthetic self, the feeling of movement and posture, occurred at times in connection with the visual self has already been illustrated. Sometimes the posture or movement was projected into the visualized self; sometimes it was felt as an attitude or movement of the actual self. At times there was an oscillation between the visual and kinæsthetic self, with no fusion of the two.

Self was, however, often projected in terms of felt or imagined kinæsthesis without visual accompaniment or with the visualization of a figure other than the self. Sometimes such attitude or movement appeared curiously objectified and at times was fused with a figure felt not to be the self.

In the following report from *D* we have an excellent example of inner or sympathetic imitation. Possibly under other than experimental conditions the organic complex would have been completely objectified and fused with the visualized posture, for it is obvious that introspective analysis may

prevent complete æsthetic absorption in the æsthetic object. The fragment concerned is one from Keats, as follows:

> Upon the sodden ground
> His old right hand lay nerveless, listless, dead,
> Unsceptred; and his realmless eyes were closed;
> While his bowed head seem'd list'ning to the Earth,
> His ancient mother, for some comfort yet.

D writes:

Perfectly clear-cut visual image of the old man in the posture described. Tactual and kinæsthetic feeling of the sodden ground. Feeling of weight and relaxation in right hand. Kinæsthetic feeling of bowed head and of closed eyes. Auditory attention, with strain in ear.

M in reporting on the same fragment suggests the possible loss of self in the object.

Put self into the old man and slight tendency to get outside and see old man.

In contrast to these reports comes *J*.

As observer I am northeast of visualized self and of old man. Visual self about one hundred feet off, looking at old man who is twenty feet farther off. No imitation of old man's posture.

The extent to which such kinæsthetic or organic imitation was felt and given either a self-reference or projected into and fused with a visual or other object of consciousness varied greatly from subject to subject. Such observations are of interest on account of their bearing upon the doctrine of æsthetic Einfühlung, particularly with reference to literary material where the object itself is imaginal, an expression in part of the reagent's own type processes, a translation in part of the poet's words.

M, who reports no case of visual self-projection and who, in general, has very vague schematic visual images, gave the highest number of empathic reports, particularly in the form of kinæsthetic empathy. He identified himself, in kinæsthetic terms, with waving flowers, palpitating trees, flying insects and the like. Thus after reading a fragment descriptive of a seashell he reports the following delicate reaction.

Emotional tone of irridescence. Einfühlung. I become blue and change to silver.

The individual differences may, however, be best appreciated by a comparison of the reports upon a couple of fragments. The first fragment, from Poe, follows:

And I rest so composedly,
Now in my bed,
That any beholder
Might fancy me dead.

The reports may be grouped as follows: (1) No self-projection; reaction impersonal. (2) Visual and kinæsthetic self-reference, with or without fusion of these two forms of self-reference. (3) Kinæsthetic self-reference, with more or less kinæsthetic self-projection.

As examples of the first group, we have the reports of *E* and *G*.

Visual image of a man asleep in bed; face very white,

and

Visual image of very old lady in bed.

A transitional form between the first and second group is found in *H*'s report:

Visual image of someone in white on a couch. Did not see face. Don't know whether it is self or not. Probably not.

The following reports illustrate the second group.

From subject *I*:

Visual image of self in bed, sprawled out; feel of posture belongs to actual body.

From *F*:

Visual image of self in bed; feel posture of rest.

J reports:

Standing near bed; visual self seen on it. Posture felt and contact from bed.

N reports:

Self seen in posture; visual, posture, and organic consciousness.

B's reports show the conflict between fusion and failure to fuse of the kinæsthetic and visual elements.

Organic uneasiness. Self seen lying on couch with tapestry cover. At word 'bed' self jumped under cover, kinæsthetic consciousness. Bed is to left. Visual as well as felt self. Felt self is at the left and yet this self is also experienced in the position of actual self. . . . Peculiar organic feeling.

The following reports illustrate the third group:

Subject L reports:

Feel the lying posture; cutaneous feel of bedclothes. No visual imagery.

From *M*:

Lying in bed. Feel another person looking at me.

K reports:

Posture and orientation consciouness. *Felt* self in middle of room, up in coffin. Self definitely projected.

The second fragment is also from Poe and reads:

> Glides spectre-like, unto his marble home,
> Lit by the wan light of the hornéd moon,
> The swift and silent lizard of the stones.

Four subjects reacted with kinæsthetic imagery, that induced to a varying degree identification with the gliding lizard.

Thus *B*.

A crawly feeling. I saw a poisonous-looking lizard writhing his way into a ruined marble hall which had grown slimy and moss-grown from age.

And *G*:

I see a lizard running among the stones on a clear moonlight night. . . . I seem to feel the movements of the lizard . . . as if I were running around on my hands and feet watching for enemies.

Of the other subjects, six projected themselves into the scene. Three of these projections were visual and show no empathic qualities.

For instance, *I* reports:

Visual of self standing, sketchy.

And *J*:

See self lying on ground. . . . Lizard thirty feet away. . . .

A and *L* get a tactual self-reference. Thus *A* reports:

A ruined wall among other ruins on a hill, the faint moonlight, the lizard. A feeling that it is a very warm still evening; the image of touching the lizard.

Of the three subjects who give no self-projection, *D*'s report is characteristic.

Visual and optical-movement image of dark lizard gliding toward broken columns, between whose shadows is seen the crescent moon.

IV

Summarizing, we may say that self-projection may occur in a form that is not empathic. A visual self-projection may be of this nature. The visualized self may be only a spectator of the scene. Such visual self-projection may become empathic when fused with it are projected kinæsthetic, tactual or organic images. That, frequently, such fusion fails is shown by those instances in which the subject feels in person

the kinæsthetic experience and does not project it into the visualized figure of the self. The objectified kinæsthetic or organic factors may, on the other hand, fuse with visual material other than that of the visual self. The visual objectification may take form as a person not the self or assume the form of an animal or some object of the inorganic world. Sometimes there occurs kinæsthetic consciousness without visual accompaniment, and such kinæsthetic consciousness may or may not be objectified.

Relative to this summary, the following questions arise: (1) What is the æsthetic value of self-projection that is not empathic? (2) Which reaction has the greater æsthetic value, the objectification of the kinæsthetic or organic resonance in a visualization of one's self or of another? (3) What value for the æsthetic reaction have localized non-projected kinæsthetic and organic sensations and images? (4) Does Einfühlung occur in cases where, apparently, there is no organic resonance whatever? Can, that is to say, a visual objectification of a movement carry the feeling-in tone, as well as, and sometimes better than, a kinæsthetic or bodily resonance?

A TIME EXPERIMENT IN PSYCHOPHYSICS

BY DARWIN OLIVER LYON AND HENRY LANE ENO

I. The Problem Stated

The work here presented grew out of a series of experiments begun in 1911 with a view of ascertaining the cause of the seeming discrepancies that various investigators found in the rate of the nervous impulse, where the results obtained varied all the way from 11 meters per second to 194 meters per second. Realizing that there was no accurate method of differentiating the time taken up in transmission from the time taken up in the cerebral reflex, we decided to use only the sensory nerves and therefore confined ourselves to these alone. While working on the musculo-cutaneous nerve we chanced upon a rather peculiar phenomenon that it is the object of this paper to discuss. Briefly stated this was, that in order to make two sensations fuse in consciousness, the more distal of the two stimuli did not have to be given at nearly so short an interval before the other stimulus, as we would have inferred if the speed of the nervous impulse generally accepted, was even approximately correct. Therefore, in beginning the series of experiments, summarized in this article, the authors were, not only, entirely unaware of the probable outcome, but they had been led, from a study of previous time-experiments, to anticipate a quite different issue from that actually obtained.

Whether the method and results here presented tend to throw some possible light upon the obscure question of the time relation between psychic processes and cortical substrate, must be left to the judgment of the reader.

II. Brief Summary of Previous Investigations on the Velocity of Nerve Conduction

It is a well-known fact that all actions of the nervous system require a certain time for their accomplishment, and

it is a matter of every day experience that the action of the senses and of the will, are not instantaneous, although they are exceedingly rapid. Though it is commonly recognized that there is a real interval between the mental decision to perform a movement and the actual execution of this movement, it is not so commonly recognized that a certain period also intervenes between a peripheral stimulus and its perception. In the case of a voluntary movement, the time consumed from the volition to the action is occupied by: (1) The act of volition,—taking place in the cortex; (2) The transmission of the motor impulse along the spinal cord and nerves to the muscle end-plates; (3) The action of the end-plates on the muscle with the concomitant, or subsequent action.

With sensations however, although there are also three processes they are not quite the same. They are: (1) Reception of the impression by the tactile corpuscles; (2) Transmission of the sense impulse to the brain; (3) The 'perception' in the brain of the 'sensation.' This may or may not be followed by action.

The first investigator to measure the rate of nerve transmission was Helmholtz.[1] He, like most of the earlier investigators, experimented with the frog, using the classical 'muscle-nerve' preparation. Helmholtz found that the period required for the excitement of the muscular fibers was 1/100 of a second. Later on Helmholtz endeavored to determine the rate of conduction in the nerve and found it to be about 28 meters per second. About the same time Marey[2] performed similar experiments in which he obtained a rate of only 11 meters per second. Lamonsky in 1880 arrived at a rate of 31 meters per second, and about the same time Bernstein got a rate of 25 to 33 meters per second.

All the above, however, refer only to the frog, and it is now well known that the speed of conduction differs in different animals. As early as 1878 Chauveau[3] performed

[1] Helmholtz, *Comptes Rendus de l'Academie des Sciences*, Paris, 1851, tome XXXIII., p. 262.

[2] Marey, 'Du Mouvement dans les Fonctions de la Vie,' 1868.

[3] Chauveau, 'Vitesse de Propagation des Excitations dans les moteurs Nerfs des Muscles de la Vie Animale, chez les Animaux mammiferes,' 1878.

experiments on both the frog and the horse to ascertain their difference in speed of transmission. For the frog he obtained a rate of 21 meters per second; for the horse 65 meters per second.

Burckhardt[1] performed experiments in 1873 on man, using an apparatus in which the beginning and the end of the nervous transmission was registered on a smoked chart. He obtained a velocity of 27 meters per second in the motor nerves of the upper and lower limbs. In the sensory nerves however, he obtained a speed of 47 meters per second. He concluded from the results of a protracted series of experiments that the transmission of voluntary motor impulses in the spinal cord was considerably slower than in the nerves, and believed it to be not more than 11 or 12 meters per second. In 1867 Helmholtz and Baxt obtained an average speed in man of 34 meters per second. Place arrived at a speed of 62 meters per second for the forearm whereas for the arm proper he found it to be about 20 meters per second. Later, Helmholtz and Baxt repeated their experiments and arrived at a rate of 65 meters per second. Contrary to the results obtained by Place, they found the speed of conduction in the median nerve to be slower below the elbow than above. Von Wittich obtained a rate of 30 meters per second.

By the reaction time method, Helmholtz obtained for the *sensory nerves* a speed of 60 meters per second. Hirsch, however, arrived at a speed of only 34 meters per second; while Richet[2] obtained a speed of 50 meters per second.

In 1893 Cattell and Dolley[3] finished an elaborate series of experiments on reaction time in connection with an investigation of the velocity of the nervous impulse. They conclude that their experiments indicate that the velocity must be considerably greater than the commonly accepted rate of 30 meters per second. They show that the cerebral reflex is almost certainly slower when the leg is stimulated than when the arm is stimulated, but that the difference in time

[1] 'Die Physiologische Diagnostik der Nervenkrankheiten,' Leipzig, 1875, p. 32.

[2] Richet, 'Physiologie des Muscles et des Nerfs,' 1882.

[3] Cattell and Dolley, 'On Reaction-Times and the Velocity of the Nervous Impulse,' *National Academy of Sciences*, Vol. VII.

of the entire reaction is much too small to allow for a rate of 30 meters per second in the nerve.

Space will not permit mention of all those who have worked along these lines, but the results obtained by certain investigators not mentioned above will be touched on later.

Our experiment, however, is not particularly concerned with the rate of the nervous impulse, and aside from the fact that our results differ from those generally obtained, our only reason for quoting the above authorities is to show how widely they differ among themselves.

Bloch[1] in 1875 concluded a series of experiments which, though not the same as ours, is along much the same lines. He devised an apparatus by which he attempted to obtain the speed of conduction in the sensory nerves, independent from that of the motor nerves. He discovered that when the finger was stimulated it brought forth a quicker response than when the cheek or the lip was stimulated. He found, when stimulating points at different distances from the brain, that the further away from the brain the point stimulated, the longer the appearance of the sensation in consciousness. He attributed this to the difference in speed of the nervous conduction from different parts of the body to the brain, and explained the quick conduction from hand to brain by saying that the impulse from the hand was capable of a more rapid transformation into a motor response than was one from the cheek for example. He also attributed this difference to a slight variation in the sensibility of the various parts of the body. By timing the application of his stimuli in such a way as to get the two stimuli to fuse in consciousness and thus be perceived at the same time, he found that the more distal of the two stimuli had to be given considerably earlier. This time interval he believed was due, and therefore proportional, to the difference in the lengths of the sensory nerves traversed. From his experiments he concluded that the rate of conduction in the sensory nerves was 192 meters per second, and 194 meters per second in the spinal cord. "His method,"

[1] Bloch, 'Experiences sur la Vitesse du Courant nerveux sensitif chez L'Homme,' *Archiv de Physiol. norm. et path.*, 1875.

says Poffenberger, "was based on the assumption that the fusion of two sensations in consciousness depends only on the time of their arrival in the center and not at all on their points of origin. His figures however, suggest that the time of appearance of a sensation in consciousness may vary according to the part of the body stimulated."

III. Description of the Apparatus Used

The apparatus can best be understood by studying the accompanying photographs and diagrams. The essential part consists of two disks of hard rubber, half an inch thick and 14 inches in diameter, revolved by clock work. The motive power is a weight of eighty pounds.

The outer disk, Fig. 2, is marked on its circumference with three hundred and sixty degrees. Each disk has a platinum contact, 1/48 of an inch wide, set in the edge. When revolving, these contacts pass two other contacts connected with a small storage battery of 8 volts, which, in turn, connects with two electrodes in such a way as to give separate shocks through each electrode as the separate disks pass the contact points (see Fig. 1 and 3).

The disks revolve upon the same shaft, and consequently at the same rate of speed; but the outer disk is adjustable, so that the contacts may be either in the same line, or set apart at any required number of degrees as graded upon the disks. In this way, by counting the number of revolutions a second, and noting the number of degrees that separate the contacts, the shocks can be given at any length of time apart necessary for the experiment. Thus, if the disks revolve once a second, and the contacts are adjusted so that the interval that separates them corresponds to 1° (1/360 of the circumference) the shocks will be given 1/360 seconds apart. If the contacts be placed 60/360 apart, 1/6 second, etc.

The speed is regulated by a brake on the shaft, and also by a second, more delicate, brake on the circumference of the inner disk.

The battery (Fig. 4) is furnished with a disk and pointer to regulate the voltage. The current is passed through sepa-

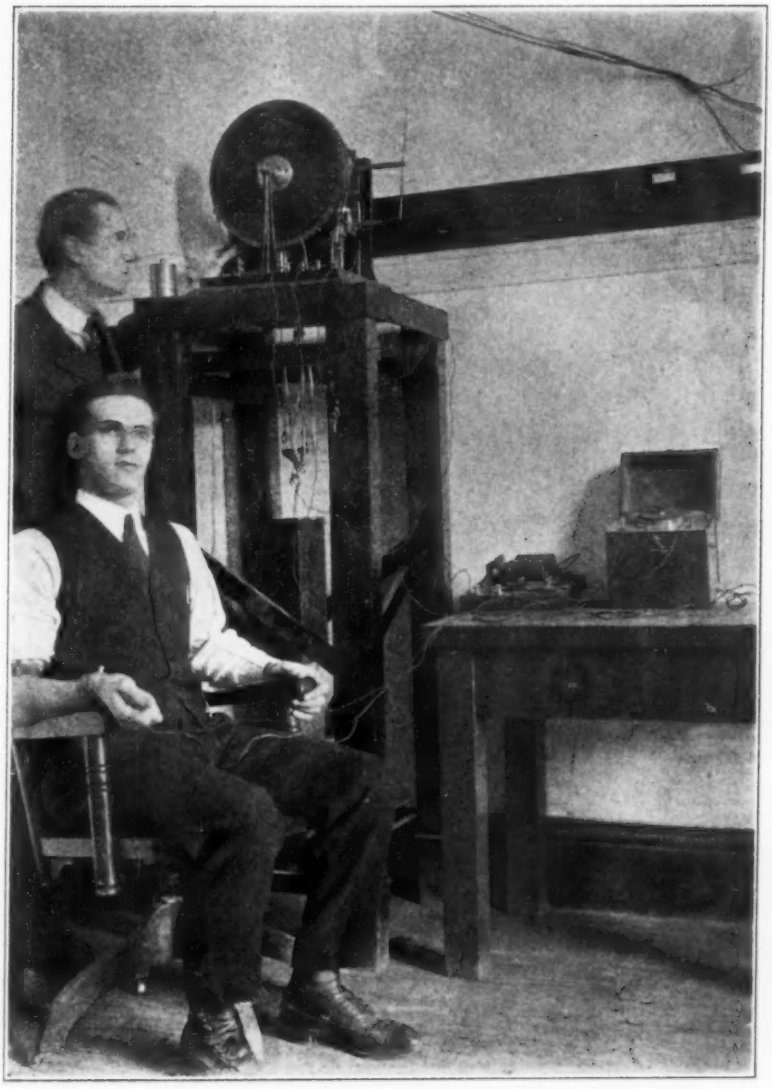


FIG. 1.

rate induction coils by which the strength of shock through each electrode can be adjusted independently as shown in Fig. 4.

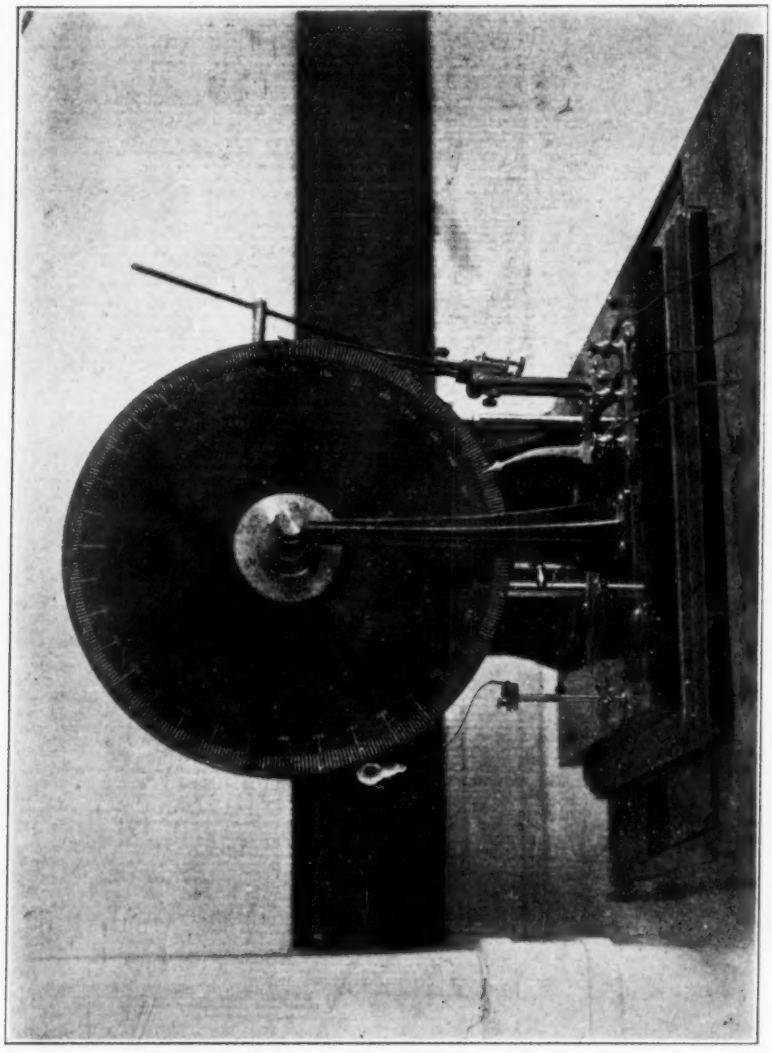


FIG. 2.

IV. METHOD OF CONDUCTING THE EXPERIMENT

During the experiment, one electrode was fastened to the wrist and the other below the elbow, as shown in Fig. 1. The lower of the two stimuli (st.[1]) was given at the wrist-joint in front of the radial artery just above the point where the nerve sends off filaments to accompany the artery as it

sinks into the wrist. The electrode giving stimulus st.^{2} was applied to the elbow-joint at the point where the musculo-cutaneous nerve emerges from the deep fascia at the outer border of the tendon of the biceps and becomes cutaneous. The position is well shown in Fig. 5. In performing the experiments the subject was seated with bared arm and in

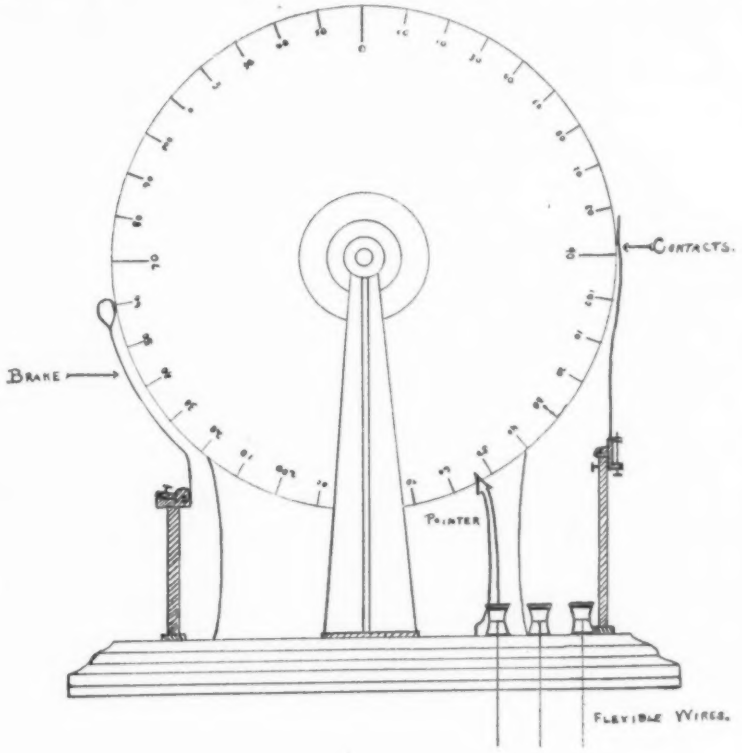
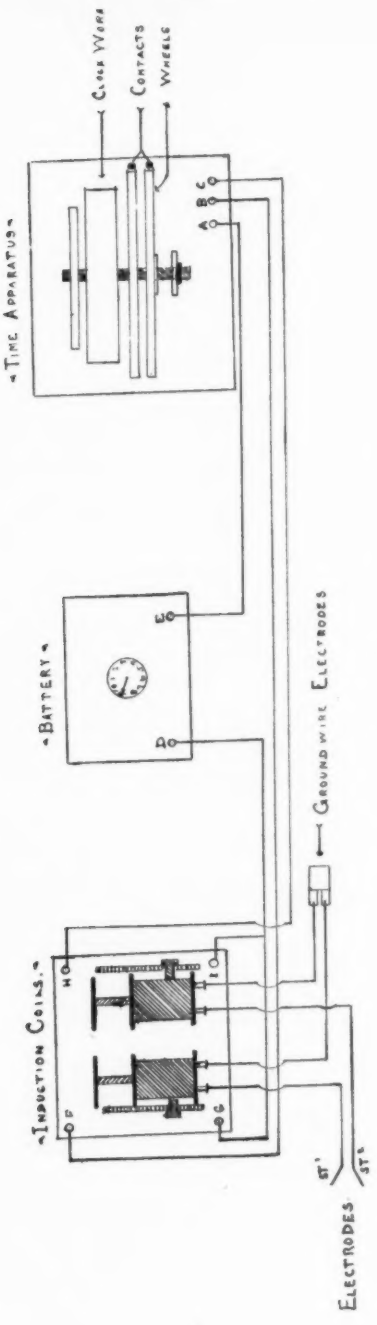


Diagram of Apparatus.

Fig. 3.

such a position that he could not see the disks. The two stimuli were then given together, when the subject felt the upper shock (st.^{2}) distinctly before the lower (st.^{1}). The disks were then gradually adjusted until the shocks were felt to occur simultaneously. In every case the time interval was noted by reading the disks.



Diagramatic Sketch Showing Method of Wiring

FIG. 4.

It was found best, at this point, to slowly move the disks still further apart until the shock from st.^{1} was felt first, and then back again, until the two shocks were felt together, thus working from both directions, so to speak, and comparing the reading of the time intervals.

Each subject was also requested to fix his attention first upon one shock and then upon the other, and the difference, if any, made by this upon the experiment, noted. The shocks were just strong enough to be felt distinctly; but, not so strong as to cause any decided muscular twitch. Before conducting the experiment proper the two induction coils were so adjusted that the shocks were felt as of approximately the same strength.

When the second stimulus was applied half way between st.^{1} and st.^{2} at the point marked st.^{a} in Fig. 5, the method was of course, the same.

V. Examination and Explanation of the Data Obtained

The experiments were started in June, 1911, first in the quiet of the country, and afterwards in the Department of Psychology of Columbia University. The uniformity of the results obtained would seem to prove that individual differences are slight if present at all. As examples we give below a few of the results taken from the notes at random.

"September 26, 10:30 A. M., temperature 62 ,subject Q. S. (clock maker). At 1/20 of a second apart[1] st.^{1} felt distinctly first. At 1/35 of a second apart st.^{1} and st.^{2} felt together.

October 3, 10:30 A. M., temperature 62, subject W. C. T. (gardener). St.^{1} and st.^{2} felt together at 1/40 of a second apart. At 1/40 of a second apart st.^{1} before st.^{a}, and st.^{1} and st.^{a} together at 1/80 to 1/90 of a second apart.

October 4, 10:30 A. M., temperature 62, subject J. G. (Swiss butler). At 1/90 of a second apart st.^{2} first, at 1/45 of a second apart st.^{1} and st.^{2} together; at 1/45 sec. apart st.^{1} before st.^{a}; at 1/90 of a second apart st.^{1} and st.^{a} together.

[1] *I. e.*, st.^{1} given 1/20 of a second before st.^{2}.

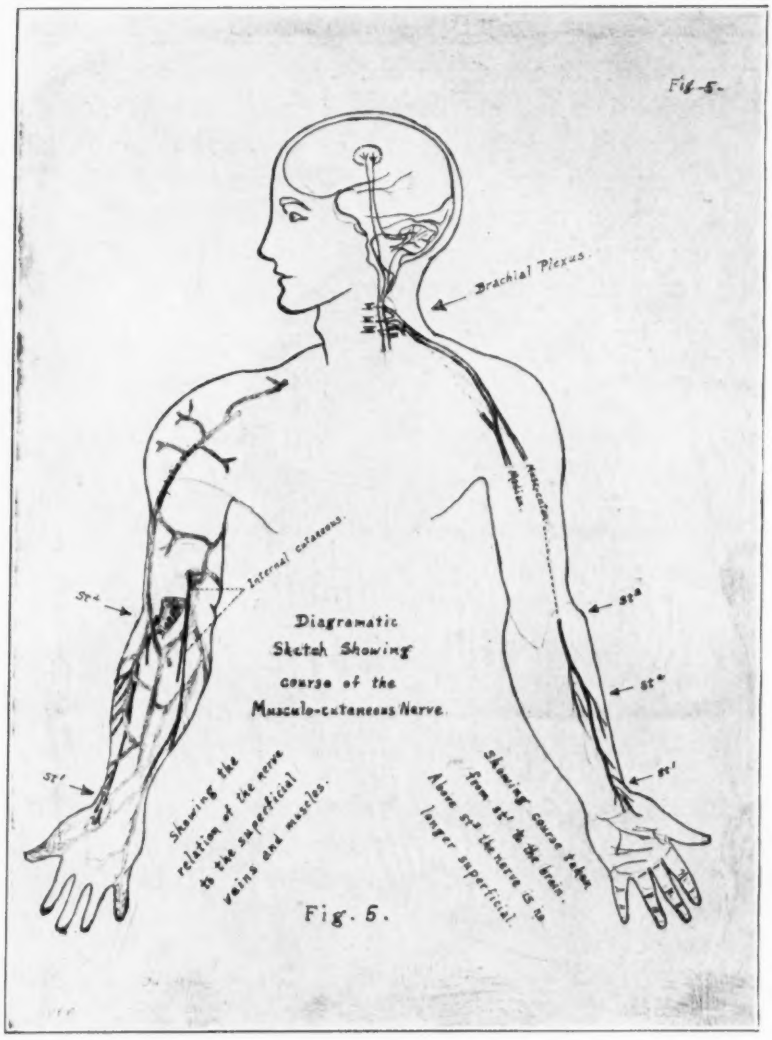


Fig-5-
Brachial Plexus.
Musculo-cutaneous
Internal cutaneous.
Diagramatic
Sketch Showing
course of the
Musculo-cutaneous Nerve.
Showing the
relation of the nerve
to the superficial
veins and muscles.
Showing course taken
from st¹ to the brain.
Above st² the nerve is no
longer superficial.
Fig. 5.

FIG. 5.

February 18, 9:00 P. M., subject A. E. R. (psychologist). At 1/80 of a second apart st.^{2} felt first; at 1/35 second apart, st.^{1} and st.^{2} felt together; at 1/40 second apart st.^{1} and st.^{2} still felt together; at 1/35 second apart st.^{1} felt before st.^{2}; at 1/35 second apart st.^{1} before st.^{a}; at 1/40 second apart st.^{1} before st.^{a}; at 1/80 second apart st.^{1} and st.^{a} felt together.

The tables given on page 325 represent only a small portion of the work performed, but they are sufficient to illustrate our case. As subjects we used various ages and classes of people. In the case of experiments performed at Columbia, the subjects were with but three exceptions, trained observers in experimental psychology, but difference in temperament or intellectual acumen seemed to have little effect.

It will be seen that when st.^{1} precedes st.^{2} by an interval of 1/35 to 1/45 of a second, the sensations occur together. For st.^{1} and st.^{a} to occur together an interval of 1/70 to 1/90 of a second is necessary.

A factor that should possibly have been considered is the actual distance between the two stimuli in the case of each subject. The differences may have been partly due to the length of arm in the different observers, since it obviously takes longer for the nervous impulse to travel the 10 inches between the two points of stimulation in a six-foot man than the corresponding 7 inches in the arm of a child, or small adult.

The results obtained from the various subjects experimented on agree so closely in the long run that instead of giving a summary of results we think it better to give merely a small but typical set of experiments (Table I.). Table II. is a tabulation of the results obtained from one subject. It will be noted that the averages obtained from this table agree closely with those of Table I. The time-intervals given in columns 3 to 9, inc., are in one thousandths of a second, and indicate the temporal relation of the sensations when the correlative stimuli are applied successively at the time intervals noted. For example, s.^{1} was felt before s.^{2} when st.^{1} was given .0412 ($\frac{1}{2}\frac{1}{0}$) of a second before st.^{2}; or again, when st.^{2} was

made to follow st.[1] by an interval of .0245 ($\frac{1}{40}$) of a second, s.[1] and s.[2] were felt to occur together. Of course columns 4 and 8 give the longest times (*i. e.*, in an ascending scale) that allowed the one stimulus to be felt before the other;—the next longer time making the sensations 'fuse.' In like manner columns 3 and 7 give the *shortest* time.

The results of this experiment as shown by the data given in the above tables, are curious, and are quite notably different from those that we should have been led to expect from our knowledge of previous experiments in reaction-time and from the experiments on the rate of the nervous impulse.

Although our experiment is not for the purpose of determining the rate of the nervous impulse, *per se*, this must be taken into consideration in a critical analysis of the data obtained. Moreover, in the case of this experiment, we consider this important, since our results seem to be at variance with those of previous investigators.

Numerous experiments to ascertain the speed of the nervous impulse have, as was explained in Section I., been carried on for many years, by various methods, and with varying results. Helmholtz as before stated, obtained a rate of about 30 meters per second for the sciatic nerve of the frog; and this rate is even yet generally considered as being approximately correct. Spencer quotes it in his 'Psychology,' and Wundt,[1] says: "The rate of the propagation of the current of action in the nerve-fiber coincides with the rate of the stimulus process itself . . . the stimulus wave, at the rate of some 32 meters in one second." We are not aware, however, of any trustworthy experiment showing a slower rate than 30 meters per second for either man or animal.

The experiments in reaction-time do not, of course, give the rate itself, but they throw considerable light, in a general way, upon the subject, by fixing a maximum limit. Some of the earlier figures for reaction-time in touch are:

Wundt	Hirch	Hankel	Exner
.201	.182	.1546	.1337

[1] Wundt, 'Principles of Physiological Psychology,' Titchener's translation, 1904, p. 80.

TABLE I.

1	2	3	4	5	6	7	8	9	10
Subject	Distance in Inches from St.^{1} to St.^{2}	S.^{1} Felt Before S.^{2} when Time Interval Was	S.^{2} Felt Before S.^{1} when Time Interval Was	Time Interval at which S.^{1} and S.^{2} were Felt as Simultaneous	Individual Deviation for Column 5	S.^{1} Felt Before S.^{a} when Time Interval Was	S.^{a} Felt Before S.^{1} when Time Interval Was	Time Interval at Which S.^{1} and S.^{a} were Felt as Simultaneous	Individual Deviation for Column 9
H. E..	8	.0412	.0095	.0245	−.0018	.0204		.0125	−.0004
F. B..	8	.0500	.0055	.0286	.0023				
F. S..	7$\frac{5}{8}$	.0500	.0083	.0333	.0070				
W. C..	8	.0411	.0080	.0250	−.0013	.0250	.0083	.0118	−.0011
J. G...	8	.0444	.0111	.0222	−.0041	.0222	.0083	.0111	−.0018
L. W..	8	.0500	.0080	.0250	−.0013	.0222	.0083	.0118	−.0011
K. H..	7$\frac{5}{8}$	.0454	.0111	.0250	−.0013	.0250	.0083	.0125	−.0004
E. E..	7$\frac{5}{8}$	.0500	.0083	.0236	−.0027	.0222		.0111	−.0018
M. I..	7	.0500	.0083	.0333	.0070	.0333	.0083	.0166	.0037
B. W..	7	.0500	.0166	.0333	.0070	.0333	.0083	.0166	.0037
A. R..	8$\frac{1}{8}$	.0450	.0083	.0277	.0014	.0138	.0083	.0111	−.0018
A. R..	8$\frac{1}{8}$	.0500	.0055	.0250	−.0013	.0166	.0055	.0138	.0009
A. R..	8$\frac{1}{8}$	.0486	.0090	.0222	−.0041	.0192	.0083	.0166	.0037
F. G..	8	.0338	.0111	.0222	−.0041	.0222	.0055	.0111	−.0018
F. G..	8	.0454	.0100	.0333	.0070	.0192	.0055	.0138	.0009
F. G..	7$\frac{5}{8}$	.0285	.0083	.0305	.0042	.0166	.0083	.0166	.0037
D. L..	8$\frac{1}{8}$	.0555	.0083	.0250	−.0013	.0192	.0055	.0138	.0009
D. L..	8$\frac{1}{8}$	.0444	.0138	.0222	−.0041	.0250	.0028	.0111	−.0018
D. L..	8$\frac{1}{8}$	.0472	.0083	.0192	−.0071	.0222	.0055	.0083	−.0046
Sum..		.8705	.1173	.5011	.0704	.3776	.1050	.2202	.0341
Aver..		.0458	.0094	.0263	.0037	.0222	.0070	.0129	.0020

TABLE II.

1	2	3	4	5	6	7	8	9	10
Subject	Distance in Inches from St.^{1} to St.^{2}	S.^{1} Felt Before S.^{2} when Time Interval Was	S.^{2} Felt Before S.^{1} when Time Interval Was	Time Interval at Which S.^{1} and S.^{2} Were Felt as Simultaneous	Individual Deviation for Column 5	S.^{1} Felt Before S.^{a} when Time Interval Was	S.^{a} Felt Before S.^{1} when Time Interval Was	Time Interval at Which S.^{1} and S.^{a} Were Felt as Simultaneous	Individual Deviation for Column 9
H.L.E.	8	.0454	.0083	.0333	.0086				
H.L.E.	"	.0454	.0083	.0166	−.0081				
H.L.E.	"	.0555	.0055	.0222	−.0025				
H.L.E.	"	.0552	.0055	.0333	.0086	.0333	.0083	.0166	.0041
H.L.E.	"	.0442	.0055	.0222	−.0025	.0222	.0083	.0111	−.0014
H.L.E.	"	.0333	.0125	.0222	−.0025	.0166	.0055	.0111	−.0014
H.L.E.	"	.0333	.0111	.0250	.0003	.0166	.0083	.0125	.0000
H.L.E.	"	.0333	.0125	.0250	.0003	.0166	.0083	.0125	.0000
H.L.E.	"	.0333	.0125	.0222	−.0025	.0166	.0083	.0111	−.0014
H.L.E.	"	.0333	.0125	.0250	.0003	.0166	.0083	.0125	.0000
Sum..		.4122	.0942	.2470	.0362	.1385	.0553	.0874	.0083
Aver..		.0412	.0094	.0247	.0036	.0198	.0079	.0125	.0012

A more recent rate gives 1/10 second from one hand to the other—a distance of more than 2 meters, so that, even without allowing for the complicated process of association, a rate of at least 20 meters per second must be posited.

Starting with this rough estimate we find, that, even if this minimum measurement of 1,260 inches—or 32 meters—per second is approximately correct, the stimulus wave should take not more than 1/116 to 1/145 second to travel from point st.[1] to st.[2]—a distance varying from seven to ten inches. According to this, therefore, the stimuli should be given at somewhere near this interval apart, in order to have the two sensations occur at the same time, *i. e.*, fuse in consciousness, but experiments show that in order to feel the two shocks as occurring simultaneously, the stimuli must be given from 1/35 to 1/45[1] second apart—a time interval three times as great.

What is the cause of this apparent discrepancy? Does it exist merely because of the method used or does the explanation lie deeper?

VI. Possible Sources of Error

As in all such experiments, the possible sources of error are legion. We enumerate below what we consider the most important.

1. Inability of the subject to perceive such minute fractions of a second, due to a fusion of successive nervous or cortical processes.

(*a*) Fusion at point of stimulation.

(*b*) Fusion between point of stimulation and cortex.

(*c*) Fusion at cortex, due to duration of excitement set up by first stimulus.

2. Inaccuracy of observation, even if such small differences can be perceived.

3. Difference in the sensitiveness of the skin between the two points of stimulation.

4. Varying depth of nerve at different points of stimulation.

[1] A fairly constant average is 1/40 second.

5. The involving of other nerves as conductors—varying at the points of stimulation.

6 The subject may concentrate his attention on one of the two stimuli, thus putting the tactile corpuscles and nerve ends in a 'hair-trigger' condition, so to speak.

7. First stimulus may seem to occur longer ahead of second stimulus than is really the case, due to 'lag,' or to the monopoly of the subject's attention aroused by first stimulus, thus setting back the apparent time of the second stimulus.

8. Previous experiments may be wrong, and the actual speed of the impulse along the sensory nerves may be much slower than supposed.

9. The first stimulus may alter the excitability or conductivity of the nerve so as to quicken or retard the second stimulus.

VII. Defense of Above Sources of Error

Taking up the possible sources of error in order, we observe:

1. Sensations occurring at the rate of several hundred a second can be distinguished as separate sensations, although, of course, they cannot be counted. This is true of both auditory and tactile sensations. Shocks 1/360 of a second apart can be distinguished as *not* simultaneous.

James quotes Exner as having distinctly heard the doubleness of two successive clicks of a Savarts wheel, and of an electric spark at 1/500 of a second apart; also Von Wittich as observing that 'between 1,000 and 2,000 strokes can be felt as discrete by the finger.' Shocks of 1/240 apart can be distinctly felt as discrete, and all the observers in the present experiment were easily able to distinguish shocks 1/120 of a second apart, as *not* simultaneous.

There cannot, therefore, be any fusion, unless the succeeding stimuli occur at a much more rapid rate than they are given in the present experiment.

And this is still true, even if a slowing up of the impulsed by the first stimulus (st.[1]) should occur in one or more plexi or synapses in its course to the brain, thus allowing that from st.[2] to catch up sufficiently to counteract or obliterate the spacial distance between the two impulses.

For were such a spacial contraction to take place to any extent (as, for example, the fusion of the two succeeding processes in adjacent neurones), it would be impossible to distinguish between such small intervals as 1/240 sec. or 1/120 sec. when two similar successive stimuli are applied to the same spot.

Furthermore, the result, as to time, would be the same whether such fusion took place all at once, at the cortex itself, or through a chain of plexi or synapses on the way thither.

But since two succeeding nervous impulses 1/120 sec. apart can readily be felt as discrete, they can not fuse either at the cortex or before.

Now as the difference between the generally accepted rate of nervous impulse of 1/120 sec. for the distance between st.[1] and st.[2], and that of 1/40 sec. as found experimentally by us is 1/80 sec., any condition, either at the cortex itself, or in the course of the afferent nerves, that would obliterate this interval, would, *ipso facto*, obliterate, still more easily, any smaller interval, and make it impossible to distinguish between successive stimuli 1/80 sec. apart, or less, which we have found not to be the case. This meets objection 1 under all three headings.

2. The question of inaccuracy of observation does not really enter into this experiment. The point is not whether such minute differences can be distinguished *accurately*, but whether they can be distinguished *at all*. Also, the margin of inaccuracy varies only within certain limits which, in this case, are wide enough to cover the objections. The fact that shocks occurring at much smaller intervals of time apart can be distinguished as *not simultaneous* covers the ground.

3. It is impossible for difference of cutaneous sensitivity at st.[1] and st.[2] to be sufficient to account for such a discrepancy in time. The difference, if any, is only slight. In the course of this experiment, no noticeable difference has been observed

4. The musculo-cutaneous nerve is at nearly the same depth at both points. It is practically subcutaneous all the way from the st.[1] through st.[a] to st.[2].

5. Although, roughly speaking, a square inch of surface involves three or four main afferent nerves, in this case any other nerves, that might be involved, are practically the same in length and course, all passing through the brachial plexus on their way to the cortex.[1] Hence, if there be a source of error here, it is practically the same for both points. Experiments, also, always show approximately the same results, even when the electrodes are not accurately adjusted.

6. Actual experiment shows that though attention may have some effect it is slight, if present at all. Attention to either point does not seem to materially vary the result. If attention has any effect, in this experiment, it seems to be limited to a possible apparent varying of *intensity* between the two shocks; not to a change of the time interval.

A typical article on this question is that upon the 'Experimental Research of the Phenomena of Attention,' by James R. Angell and Arthur H. Pierce in *The American Journal of Psychology*, Vol. 4, page 528, following Wundt's experiments. Another is James's criticism of these experiments in his 'Principles of Psychology,' page 415, Vol. 1, in which the possibility of correctly determining simultaneity between the ring of a bell and the position of a pointer were investigated. The results of all similar experiments go to show that there is a complicated margin of error, both positive and negative, due, apparently, primarily to attention, and possibly other factors.

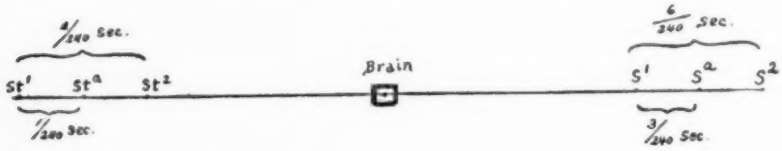
All these experiments, however, seem to have been carried on between different kinds of sensations, as, for example, sight and touch, or series of sensations in rapid succession. This source of error, however, does not seem to apply where only two successive sensations are in question. The remarkable uniformity of our results in the present experiment would appear to be *prima facie* evidence that such a source of error does not play an appreciable part in this case.

[1] In the case of st.[1] the only other nerve that might by accident receive the stimulus would be a cutaneous branch of the median. On the radial side of the forearm we seldom find branches of the internal cutaneous extending so low as the wrist. In the case of st.[2] the only other nerve in the vicinity is the musculo-spiral. We can often tell if the electrode is in contact with a filament of this nerve by observing the nature of the muscular contraction when the stimulus is increased.

It is well known to the trained musician that the very minute fraction of a second between two tones on the same instruments (*e. g.*, two violins), where these two tones are supposed to be simultaneous, can be easily determined, particularly in some of the older music, like Bach, where violin runs of fifty or sixty notes a second, are by no means uncommon, and must be performed with the greatest accuracy.[1]

7. This monopoly of the observer's attention seems, at first, a serious objection, and a possible source of error undoubtedly here exists. Two things, however, may be said by way of defence. In the first place there is no doubt but that the error is minimized when only *two* successive stimuli are used, since nearly all of the experiments of a similiar nature have been performed with *series* of rapid stimuli.

In the second place, such an apparent "lag" of the second sensation behind the first cannot be great enough, in the present instance, to invalidate seriously the results of this experiment, as will appear from the following diagram.



For, if the actual time between st.[1] and st.[2] is 2/240 second (nerve rate, being taken at 32 meters per second), and yet the difference in time between resultant sensations s.[1] and s.[2] is 6/240 second, this "lag" would be 4/240 second. Therefore, if the actual time between st.[1] and st.[a] is 1/240 second, the apparent difference between sensations s.[1] and s.[a] should be 5/240 second (1/240 + 4/240), since the "lag" would be practically the same in both cases. Whereas, by experiment, we get apparent time st.[1] − st.[a] = 3/240 second, which shows conclusively, not only that "lag" alone cannot account for the

[1] As a matter of fact, no difference in results due to varying attention was found in this case; and we understand that, although Prof. Münsterberg found a difference in the apparent strength of different shocks due to attention, more recent experiments carried on at Columbia have not tended to confirm his results.

discrepancy, but that "lag," in this instance, hardly affects the experiment at all. The classical "complication experiment" does not enter here since we are not using different kinds of stimuli.

8. Even if the minimum rate, usually given, of 30 meters a second is incorrect, it could not be so slow as 10 meters a second, which is what the present experiment would indicate.

The ordinary reaction experiment gives an average of 2/10 second for time from stimulus to conscious reaction (1/10 from hand to hand is a minimum). During this time, says James, "(1) The stimulus excites the peripheral sense-organ adequately for a current to pass into the sensory nerve; (2) The sensory nerve is traversed; (3) The transformation or reflexion of the sensory into a motor current occurs in the centers; (4) The spinal cord and motor nerves are traversed; (5) The motor current excites the muscle to the contracting point."[1] Besides all of which, as a sixth point, 'the central elements offer incomparably more resistance than the nerve fibers to the progress of an excitation . . . a retardation of conduction, amounting on the average to 0.003 second, occurs in the spinal ganglia of the frog.'[2] In the light of all that is now known on the subject we feel safe in saying that the 'nerve-current' in all probability moves at its greatest speed over the comparatively unimpeded nerves before the medulla and complicated ramification and decussations of the brain are reached. Where delay exists it undoubtedly occurs in the gray matter and may conceivably be due to the slower conduction in the minute conducting elements such as the dendrites, perikarya, arborizations, etc. Wherever one neurone ends and another starts, *i. e.*, wherever a synapse occurs, delay may be theoretically inferred, but it is extremely improbable that any greater number of synapses are involved when the stimulus is given at st.[1] than when it is given at st.[2].

As a result of an extended series of reaction experiments Cattell and Dolley say, 'we believe—that the velocity is greater than 30 m. per sec.'[3] and again, in the PSYCHO-

[1] James, 'Psychology,' Vol. 1, p. 88.

[2] Wundt, 'Physiological Psychology,' Titchener's translation, Vol. 1, p. 88.

[3] Cattell and Dolley, 'On Reaction-times and the Velocity of the Nervous Imuplse,' National Academy of Sciences, Vol. 7.

LOGICAL REVIEW for March, 1894, page 167, Cattell reaches the same conclusion finding 'a velocity of the nervous impulse in the sensory tracks of the spinal cord of about 40 m. per sec.' Several of the more recent investigators have found even a more rapid rate.[1]

The 'nerve-current' therefore, must move at a much greater rate than ten yards a second (1/40 second for the eight inches between st.[1] and st.[2]), or it could never reach the cortex, pass through all the retardations and transformations enumerated above, and get back to the hand during the time that the reaction experiments show.

It is evident therefore that some other explanation must be given for the apparent discrepancy.

9. That the passage of an electrical current through a nerve causes profound changes in both its excitability and in its conductivity is a conceded phenomenon. The condition is known as *electrotonus*. In view of the fact that a similar condition is caused by a simple electric shock, it is possible that the explanation lies here. It should be borne in mind, however, that two excitatory states recurring at 1/2,000 sec. interval can be propagated along the nerve without blending, *i. e.*, the upset of nerve equilibrium by the first stimulus does not interfere with the production of a fresh disturbance by a second stimulus even though only 1/2,000 of a second has elapsed. It is quite possible, that the altered condition of the nerve is sufficiently profound and lasts long enough to cause a retardation of the second stimulus, but the same could be said if the interval were 1/120 instead of 1/40 second. An overlapping is of course possible, but the question is *how far* the response to a second stimulus is prohibited because a previous stimulus is still in progress, or because the nerve has not yet 'recovered.'

[1] Hermann, 'Handbuch der Physiol.,' Vol. 1, Pt. 1, p. 23; Kiesow, 1903, *Ziet. fur Ph.*, Vol. 33, 444; Piper, 1908, *Pfluger's Archiv*, 1908, Vol. 124, p. 591; Chauveau. 'Vitesse de Propagation des Excitations dans les moteurs Nerfs,' etc., 40–75, *Compt, rend. Acad. de Sc. Par.*, 1878, 87, 95, 99, 138–142; Helmholtz and Baxt., 65 M., *Monatsber. der Berliner Acad.*, 1870, 3, 184; Bloch, *Arch. d. Physiol. norm. et path.*, 1875 (Bloch places a rate of 192 m. per sec. in the nerves, and 194 in the spinal cord.) Richet, 'Physiologie des Muscles et des Nerfs,' 1882; Hall and Kries, *Archiv fur Anat. und Physiol.*, 1879, Sup. 1–10; Exner, 'Entwurf z. einer physiologisch Erklärung, 1894.

VIII. General Argument and Discussion of Results

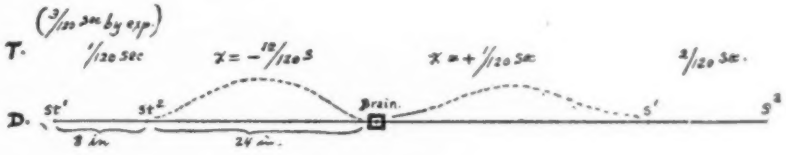
If the results of the experiment described in the foregoing pages are correct, and if no error exists in the apparatus used, we are faced with the apparently anomalous fact that, in order to obtain simultaneous sensations from the two stimulation points in question, the stimuli must be applied at such intervals as to cause them to reach the appropriate cortical center in the neighborhood of 1/60 second apart. We may therefore conclude that:—either such short time intervals cannot be distinguished, which we have shown is not the case; or the sensations (or whatever immediately gives rise to them) do not occur simultaneously in the brain at all. Yet the processes must occur practically together *somewhere*, in order to enable the concomitant sensations to occur together. Where they occur is another question.

Again, there must exist *some time interval* between the cortical process (the brain state) and the occurrence of the allied psychic process (the sensation) since it is not probable that one sensation can be synchronous with the cortical process, and the ensuing sensation come after it.

A time interval in one case necessarily presupposes a time interval in the other case.

And if so, what is the relation between them?

The following diagram illustrates the case graphically.



Time st.^{1} − st.^{2} = 1/120 sec. (30 yards a second).

Time st.^{2} − $B(x)$ less than 12/120 (1/10) second, since reaction experiment shows at most 2/10 for st^2 − B − st.^{2}.

But difference of st.^{1} − st.^{2} = 3/120 second (1/40). Therefore s.^{1} and s.^{2} must occur synchronously at some such place as s.^{1} or s.^{2}.

The same holds true when st.^{a} is applied at half the distance between st.^{1} and st.^{2}, giving the same proportionate result.

It will be observed that time st. $-$ B is unknown (x), except that it is, presumedly, not more than 1/10 second, since the ordinary reaction experiment gives 2/10 second as the average time. In the same way, Time $B - S$ is unknown. One of the main things, therefore, in this experiment, is to discover whether such a time interval exists at all.

The foregoing experiment would seem to indicate, not only that such a time interval *does* exist between the cortical process and the ensuing sensation, but that the interval is probably of a measurable duration. In the case of a sensation, arising from a stimulus applied at the wrist, the time that elapses between the arrival of the stimulus-wave at the cortex and the following sensation would seem to be in the neighborhood of 1/60 second—or if the ample margin of one half of the apparent interval be allowed for the possible sources of error, 1/120 second—an interval by no means so small as to be imperceptible or unmeasurable.

IX. Conclusion

In summing up the results of this investigation we desire to again call attention to the fact that the experiment is not one for the determination of the speed of the nervous impulse, *per se*, for we are not concerned as to whether the speed is 20 or 200 meters per second. Had we been desirous of knowing the exact speed, and of *using* the same in our computations, a different apparatus would have been necessary.

The more probable of the many sources of error that are necessarily entailed in an experiment of this nature have been discussed in Section VI. These possible errors were discussed and defended to the best of our ability in Section VII. but the authors make no claim that their "defense" is conclusive. They realize that not only should the length of nerve traversed be considered,—but the number of synapses as well, and they further realize that even though the number of synapses be the same, and even though the sensitivity of the two points chosen be equal, that the *frequency* with which the nerve is accustomed to transmit tactile sensations is a most important factor.

In this connection we desire to make mention of a factor that was not considered in Section VI., since we do not consider it a source of error in any sense of the word. It is one, however, that needs consideration; we refer to the fact that with certain nerves, only *certain kinds* of stimuli will elicit a response. Many instances can be cited in which particular reflexes can be elicited only by certain stimuli. A very characteristic reflex in the cat is the pinna-reflex. When the pinna is tickled or touched in a certain way it twists so that its free end is turned backward. This reflex is elicitable by various *mechanical* stimuli but it is impossible to provoke it by any form of *electrical* stimulation.[1] Here, as in the case of the reflex-arc, and notably in the organs of *sense*, it is probable that the terminal nerves and tactile corpuscles are "attuned," so to speak, to respond only to stimuli of a certain definite kind. We may say that the chief function of the 'organ' or 'receptor' in which the reaction starts is to convey to the central nervous system sensations of one kind, and of one kind only, and it is the function of the 'receptor,' be it a Pacinian corpuscle, an end-bulb of Krause, or a plain tactile corpuscle, not only to lower the threshold of excitability for a certain type of stimulus but to heighten it for all other types, these stimuli not being such as the mechanism is adapted for. We think it extremely improbable, however, that the musculocutaneous nerve is any better adapted for electrical stimuli at the elbow than it is at the wrist. If there is any difference it must be one of degree and not of kind, but experiment would seem to prove that there is but little difference in the tactile sensitivity of the two points.

The authors realize that the possible error No. 7 given in Section VI. is a serious one, and that it is one, which, in consideration with the relative use of the parts, *i. e.*, the *frequency with which the nerves are accustomed to transmit tactile and other stimuli*, needs further consideration. But whether the 'apparent discrepancy' is explainable here, or elsewhere, the experiment, unless the apparatus is at fault, offers a contribution, small though it may be, to the laws underlying nervous activity.

[1] Sherrington, 'The Integrative Action of the Nervous System,' page 10.

Finally, unless a purely mechanical or physiological explanation is offered, we offer a possible contribution to the doctrine of psychophysical parallelism. This has generally been left to the armchair-philosopher, and thus far the experimental psychologist has contributed little if anything. The experiment that it is the object of this paper to present, can, however, here offer something that may prove suggestive. For we are confronted with the seemingly anomalous fact that in order to get two sensations to 'fuse' they have to be applied at an interval of about 1/40 of a second, whereas according to the current theory the interval should not have to be even one third of this. It would seem that whatever the relation of cortical process and corresponding sensation may be, it is not one of simultaneity, and speaking temporally we might even say that s.^{1} and s.^{2} do not occur together in the brain when they are felt as synchronous.

In conclusion we wish to say that although the method of conducting the experiment, as described in Section IV. was, at the time, considered the best possible, we have come to realize that what is known as the "method of right and wrong cases" would have been better. It is the one that we shall use in the future, either alone or in conjunction with others. With this method we shall so *fix* the disks that shock st.^{2} will follow shock st.^{1} after an interval that will be too small to be noticed each and every time, but not so small that it will not be noticed *most* of the time. Thus with this method we can ascertain by the percentage of right judgments the subject's degree of perceptibility. Having found the percentage of right cases given by several subjects with the contacts separated at a certain distance, we could then compute from this, the distance that the contacts should be separated in order to obtain any percentage desired of right cases. We propose a solution of this rather intricate problem in the near future, by a combination of various methods, and by using tactile as well as electrical stimuli.

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